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SUBJECTIVE VAL[illegible] [illegible]RMATION

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Research into the way information is processed has indicated that it is undervalued. There are suggestions that this may be the result of central mental processes, response bias or the method of measuring subjective probability. The research described here used confidence ratings and decision times as independent indices of subjective probability. These two measures were found to correspond closely in the findings. The results show two distinct effects: one is to overestimate the probability of unlikely events (Cohen's inertial-ψ effect) and the other is to underestimate the value of information in changing the probabilities (a 'conservatism' effect). However, not only is it the case that information is undervalued, but the relationship between information and subjective probability is different from the relationship between information and statistical probability. Subjective probability appears to increase linearly with the antilog of the information in bits. The possible meaning of this result is discussed.

The way in which information is evaluated subjectively has not received as much attention as research on the psychological aspects of other stimuli (e.g. weight, time, length). Cohen & Christensen (1970) have suggested that it has been implicitly assumed that binary units of information are essentially psychological entities. Earlier, Cohen & Cooper (1962) attempted to investigate the way information is subjectively processed. They found that the subjective value of a bit of information was not equivalent to its objective value. Their technique involved asking subjects to guess how many 'yes or no' questions they would need to ask to find the one circle the experimenter was thinking of, out of a page of circles. Other subjects actually tried to locate the circle by the same method of questioning, and the number of questions needed to succeed was counted. The number of questions estimated to be necessary was greater than the number actually used, but both were greater than the number of bits of information on the page.

The method of Cohen & Cooper provides only an indirect examination of the subjective value of information. Their results may be influenced by an appreciation, or otherwise, of the logic of asking binary questions, and may be more affected by cognitive sophistication than subjective evaluation of information.

In some experiments (e.g. Cohen *et al.*, 1971, 1972) subjects have overestimated the probability of events: subjects have extracted far more certainty than the information presented to them warranted. Typically this has occurred when subjects derive an opinion about an unlikely event on the basis of information given to them in one piece, as a complete description of a situation. (This might be like the derivation of the initial *a priori* estimate to put into Bayes's theorem.)

Other research (Edwards *et al.*, 1965; Phillips & Edwards, 1966) on the revision of subjective probability in the light of new information, has indicated that humans are typically conservative and undervalue information in the revision of opinion. Some of the influences which affect the degree of conservatism are summarized by Sanders (1968). Explanations have been suggested in terms of misaggregation and misperception which are discussed by Edwards (1968). DuCharme (1970) has shown that conservatism in information processing can result from a bias against responding with extreme probabilities or odds. To call this effect 'response bias' suggests that it is essentially a feature of the way responses are made, whereas it may be a reflexion of intuitive states of mind. Geller & Pitz (1968) measured confidence and decision speed as subjects received information, and their results indicated that the failure to revise opinion as a result of new information may be due, in part, to the method of measuring confidence. Relative changes in decision speed reflected changes in objective probabilities more closely than changes in confidence ratings. Unfortunately it was not possible to calculate from decision speed the extent of the revision of opinion because decision speed was not calibrated in terms of subjective probabilities.

The present research was designed to investigate the relationship between binary units of information and the subjective use made of them. It was expected to find a psychophysical type of relationship for the discrepancy from the objective value of information. The method employed was to measure changes in subjective probabilities as successive binary units of information were given. The changes in subjective probability were then compared with the changes that would be expected on the basis of statistical probability.

In view of the possible effects, mentioned above, of the method of obtaining responses, subjective probabilities were assessed by taking confidence ratings and decision times which were calibrated to provide scales of subjective probability. Decision time (and similarly confidence rating) is only an index of subjective probability: in itself decision time is not a measure of subjective probability because the units are those of time. However, because there is a regular relationship between decision time and subjective probability (Cartwright, 1941; Cohen *et al.*, 1960; Branthwaite, 1974) the latter can be derived from the former by reference to graphs relating the two. Decision times from different subjects need to be calibrated individually because a given time may be relatively long for one subject but short for another: it is a scale of variable length for different subjects. So, in order to interpret the absolute values of the subjective probabilities, a preliminary experiment was necessary to calibrate the decision time and confidence rating scales of each subject.

METHOD

Subjects

The 30 subjects were male undergraduates in their first year at university.

Apparatus and procedure

Calibration experiment. The calibration experiment used apparatus similar to that of Cohen *et al.* (1960) except that times were recorded automatically. Subjects stood or sat as they preferred at one end of a baize-covered table 61 cm wide, with raised edges. Towards the other end, 107 cm away, there were sliding shutters which could be adjusted to leave a gap of various widths at the centre of the table. The widths of gap presented were: 1·27, 3·18, 4·45, 5·72, 6·99, 8·89, 12·70 and 25·4 cm. In front of the shutters was a screen which could be lowered to conceal the shutters while alterations were made to the width of gap. The gap was presented by raising the screen which automatically started a chronometer.

The task was ostensibly to roll a ball (1·73 cm diameter) through the gap, but subjects were first asked to make predictions of how well they would perform. On each trial, subjects were asked to make predictions in one of three ways: (i) by deciding whether they could roll the ball through the gap presented, and replying 'yes' or 'no' by pressing one of two buttons correspondingly labelled. Pressing either button stopped the chronometer and the time to decide was recorded as well as the decision; (ii) by making a similar decision and then rating how confident they were that the decision was correct. Ratings were made on a 10 cm undifferentiated line, labelled 'no confidence that decision is correct' at the left hand end, and '100 per cent confidence that decision is correct' at the right hand end; (iii) by estimating how many out of ten balls they could successfully roll through the gap. Only one method of prediction was used on each trial so each width of gap was presented three times; the order of presentation of the gaps and the method of prediction were randomized. Subjects were not told that decision time was being recorded.

Apparatus and procedure

Principal experiment. The apparatus consisted of 10 cards with a matrix of 32 × 16 squares drawn on each. The squares were numbered from 1 to 32 along the top and lettered from A to P down the left hand side. On the first card, all 512 squares in the matrix were blank, on the second card, half the squares (256) were obliterated, and so on: each successive card had half the number of squares on the previous card obliterated. So the tenth card had only one square which was undeleted. Each card in the sequence gave one bit of information by reducing the area of uncertainty by half.

Subjects were shown the first card and told that one of the squares had been chosen and marked on a duplicate card which the experimenter had. The task was to decide which square had been chosen. To help in making the decision, information would be given by means of successive cards with half of the remaining cells eliminated. Each time a card was shown, subjects were asked to say whether they thought they could decide correctly which cell had been chosen. Subjects replied 'yes' or 'no' by pressing appropriately labelled buttons. The cards were presented in a tachistoscope (but they were not presented tachistoscopically) so that the time from the presentation of the card to the 'yes' or 'no' reply could be accurately measured to provide an index of the subjective probabilities. Subjects were not aware that decision time was being recorded. After making their reply, subjects rated their confidence in being able to decide correctly which cell had been chosen, using rating scales as in the Calibration Experiment. All ten cards were shown to subjects.

Analysis of data

From the data of the calibration experiment, graphs were plotted for each subject of confidence ratings against estimates out of ten, and decision times against estimates out of ten.

These graphs of decision times and confidence ratings plotted against estimates out of ten had inverted-V and V relationships respectively. Decision times and confidence ratings have to be interpreted in conjuction with the decision 'yes' or 'no'. Decision times and confidence ratings for decisions which were 'no' correspond to subjective probabilities from 0·0 to approximately 0·5 (roughly the left hand side of the V or inverted-V) and affirmative decision times and confidence ratings correspond to subjective probabilities above that.

The confidence ratings and decision times of the principal experiment were converted into their equivalent estimates out of ten by reference to the calibration graphs of individual subjects.

In the case of decision times, the graphs referred to above were plotted using standard scores. The reason for using standard scores was that decision time comprises three components: reaction time, information processing time, and response latency which is the index of subjective probability (Hodge & Lanzetta, 1968). However, the information processing time was not constant in the situations employed in the calibration and principal experiments. The time taken to process information from a gap at the end of a table is unlikely to be equal to the time taken to process information from a matrix with a proportion of squares eliminated. So the conversion to standard scores was used to compensate, to some extent, for these differences in information processing time between the two experimental situations (calibration and principal experiments). After conversion to standard scores, the response latencies would be apparent as fluctuations about a base line, the base line being the mean decision time within each experiment. This made some allowance for the difference in information processing time between the experiments. However, some results of the principal experiment still fell outside the range obtained in the calibration experiment.

Table 1. *Mean subjective probabilities of correctly guessing the chosen square after successive bits of information*

Bits of information supplied	0	1	2	3	4	5	6	7	8	9
Mean subjective probability derived from decision times	0·07	0·12	0·16	0·23	0·33	0·41	0·39	0·50	0·60	0·80
Mean subjective probability derived from confidence ratings	0·03	0·06	0·09	0·14	0·21	0·33	0·42	0·46	0·67	0·99
Statistical probability	0·002	0·004	0·008	0·016	0·031	0·063	0·125	0·25	0·50	1·0

RESULTS

The average results for the 30 subjects are shown in Table 1. The way in which subjective probabilities increased with successive bits of information was not linear, but a positively accelerated function.

The data in Table 1 indicate a peculiarity in the decision time measure of subjective probability. When the ninth bit of information was presented, only one square – the chosen square – remained uneliminated. Subjects could at this point be absolutely certain as to which was the chosen square. This is reflected in the confidence rating measure of subjective probability (0·99). However the decision time measure indicates a subjective probability of only 0·8. This result might indicate that the subjective probabilities derived from decision times are inaccurate. The conversion into standard scores may not have been sufficient compensation for the increase in information processing time involved in the principal experiment. This would argue that the subjective probabilities derived from decision times are underestimates. But Table 1 shows that the decision time measure does not consistently underestimate the confidence rating measure. An alternative explanation, at least as likely, is that the final experimental situation, when all the information was presented, gave rise to an artifact in the decision time measure. Subjects had still to respond 'yes' or 'no' to the question about whether they could decide correctly which cell had been chosen. Since it was obvious which cell had been chosen, many subjects were surprised by the redundancy of the question and this delayed their response time, and artificially depressed the subjective probability as measured by this method.

Various transformations were tried to obtain a simple straight-line function between subjective probability and amount of information. The plot, shown in Fig. 1, of subjective probability against the antilogarithm of the information (in bits/10) was found to be closest to a straight-line function. (Taking logarithms of the Y axis was found to be less satisfactory.)

The equations for the two functions are:

Subjective probability (derived from decision time)
$= 0{\cdot}97$ (antilog of (number of bits/10)) $+ 0{\cdot}02$;

Subjective probability (derived from confidence rating)
$= 1{\cdot}31$ (antilog of (number of bits/10)) $- 0{\cdot}11$.

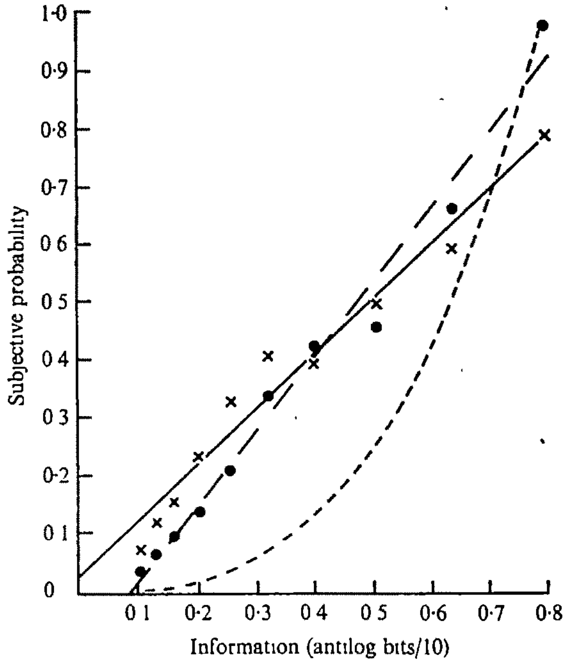

Fig. 1. Mean subjective probability after each bit of information was presented. ×—×, subjective probability derived from decision times; ●—●, subjective probability derived from confidence ratings; - - -, statistical probability.

The correlations between the axes are 0·98 for subjective probabilities derived from decision times, and 0·99 when derived from confidence ratings. The antilogarithmic function suggests that, when there is a great deal of uncertainty in the situation, the early bits of information are given subjectively less weight. The possible meaning of this relationship is dealt with further in the Discussion.

Discussion

The results of this experiment seem to involve two distinct effects. One effect is that subjects overestimate the probability of unlikely events (statistical probability less than 0·5). The subjective probabilities, in Table 1, of being able to correctly locate the square chosen by the experimenter, are greater than the statistical probabilities up to a statistical probability of 0·5. The phenomenon of overestimating the chance of success at difficult tasks, and also underestimating easy tasks, has been pointed out by Cohen (1960). In a more recent series of experiments, Cohen *et al.* (1971, 1972) have quantified the relative overestimation of compound probabilities in a situation involving sequential choices. They have termed this overestimation the 'inertial-ψ effect'. It seems, however, from the graphs accompanying their results, that this effect is part of the previously noted double phenomenon of overestimating difficult tasks and underestimating easy tasks.

The present research involved measuring probabilities in a much simpler situation, but the results can be treated similarly to Cohen *et al.* when we find that the relative overestimation is given by the equations: for subjective probabilities derived from decision time:

$$\log_{10}\frac{\text{subjective probability}}{\text{statistical probability}} = 0{\cdot}19\ (\text{bits of information in stimuli}) - 0{\cdot}06,$$

for subjective probabilities derived from confidence ratings:

$$\log_{10}\frac{\text{subjective probability}}{\text{statistical probability}} = 0{\cdot}14\ (\text{bits of information in stimuli}) + 0{\cdot}05.$$

The slopes of these functions are less than those found by Cohen *et al.* so that the degree of overestimation of unlikely events seems to have been less in this simpler task, where the probabilities were not so extreme.

While the overestimation of unlikely events found in the present results is consistent with previous research, it could be argued that the overestimation is due to the insensitivity of the confidence rating scale: subjects could not mark off an interval less than one millimetre so they could not express a probability less than 0·005, except for zero. Subjects could (and some did) rate their confidence at zero over the first few bits of information, so that it was practically possible for subjects to underestimate the objective probabilities. Further reassurance that the result is meaningful comes from the use of decision times. The confidence rating and decision time indices of subjective probability closely correspond in these findings. Because decision time was taken without subjects' awareness it is relatively free from conscious influence and bias. While subjects may have felt constrained to show an increase in their confidence ratings as more information was supplied, this pressure is unlikely to have affected the subjective probabilities derived from decision times.

The second effect found in the data is an underestimation of the influence of information in changing probabilities.

An examination can be made of the way information is subjectively evaluated by comparing the changes in subjective probability with the changes in statistical probability as successive bits of information were given. A bit of information is defined as that amount which reduces the statistical uncertainty by half. It can be seen from Table 1, that the value of a bit of information is subjectively underestimated because subjective probability does not double with successive bits of information, but increases at a slower rate. However, not only is it the case that information is under-valued, but, what may be more important, is that the relationship between subjective probability and information is different from the relationship between statistical probability and information, as defined by a bit. Fig. 1 shows how subjective probability increases linearly with the antilog. of the information. Again this result is indicated by both measures of subjective probability. Bearing in mind that Geller & Pitz (1968) found decision speed to be more sensitive than confidence ratings in reflecting changes in objective probability, it is especially important that the decision time measure shows the effect described.

The two effects found to be operating on the way subjects processed information, can be thought of as separate but compatible. The overestimation of unlikely events (Cohen's inertial-ψ effect) can only become an underestimation of likely events if there is an underestimation of the change in probabilities as information is supplied (an effect often called 'conservatism'). The underestimation of the value of information appears, from this research, to be related to the antilog. of the number of bits. Subjects seem to treat logarithmic changes as if they were arithmetical. From subjects' comments, it appears that attention is focussed on the number of possibilities at each stage, rather than the change from one stage to the next. After each of the first few bits of information were given, subjects were still confronted with a seemingly overwhelming range of possibilities. Only over the last four, or so, bits of information did the actual reduction in the number of possibilities have an impact, but even here most interest was centred on scrutinizing the options that were left. Attention appears to focus on the absolute number of event possibilities available after information has been given rather than the relative change that has taken place. The antilogarithmic relationship found suggests that at higher levels of uncertainty we are progressively indifferent to the change that information makes to the number of possibilities. It seems from this that bits are not a psychological unit of measurement, and any attempt to understand the way information is used and processed (in decision-making, for example) needs to take into account the subjective value of information, which seems more directly related to the antilog of the number of bits.

There is one other feature of Fig. 1 which deserves some comment. Inspection of Fig. 1 reveals that the relationship between subjective probability and information may not be adequately represented by a single straight-line, stretching from zero subjective probability to certainty. It is possible that the plot derived from decision times and the plot from confidence ratings could both be described better by two lines, of slightly different slopes, the breaks occurring at subjective probabilities of about 0·4–0·45. This may be the point referred to earlier in the discussion, where subjects became more aware of the actual change in the number of possibilities with each bit of information. This is an interesting effect and it is reassuring and convincing that both measures of subjective probability (derived from decision times and confidence ratings) show this characteristic. However, the two measures do not always correspond and confirm each other. In the case of the function derived from decision times, the slope between subjective probabilities of 0·0 and 0·45 is greater than the slope between 0·45 and 1·0. In the case of the function derived from confidence ratings the lower slope is less than the upper slope. This disparity in the two measures prevents any certain explanation, and it may be an experimental artifact. But it is suggestive for further work that there may be a change in information processing whereby the rate of decrease of subjective uncertainty is different from the rate of increase of subjective certainty.

I am grateful for the suggestions and advice of Dr J. O. Robinson. This study was conducted as part of research for a higher degree.

REFERENCES

BRANTHWAITE, A. (1974). A note comparing three measures of subjective probability, their validity and reliability. *Acta psychol.* (in press).

CARTWRIGHT, D. (1941). Relation of decision-time to the categories of response. *Am. J. Psychol.* **54**, 174–196.

COHEN, J. (1960). *Chance, Skill and Luck.* Harmondsworth: Penguin Books.

COHEN, J., CHESNICK, E. I. & HARAN, D. (1971). Evaluation of compound probabilities in sequential choice. *Nature, Lond.* **232**, 414–416.

COHEN, J., CHESNICK, E. I. & HARAN, D. (1972). A confirmation of the inertial-ψ effect in sequential choice and decision. *Br. J. Psychol.* **63**, 41–46.

COHEN, J. & CHRISTENSEN, I. (1970). *Information and Choice*. Edinburgh: Oliver & Boyd.

COHEN, J. & COOPER, P. (1962). Subjective value of a 'bit' of information. *Nature, Lond.* **196**, 360–361.

COHEN, J., HANSEL, C. E. M. & WALKER, D. B. (1960). Time taken to decide as a measure of subjective probability. *Acta psychol.* **17**, 177–183.

DUCHARME, W. M. (1970). Response bias explanation of conservative human inference. *J. exp. Psychol.* **85**, 66–74.

EDWARDS, W., LINDMAN, H. & PHILLIPS, L. (1965). Emerging technologies for making decisions. In T. H. Newcomb (ed.), *New Directions in Psychology* II. New York: Holt, Rinehart & Winston.

EDWARDS, W. (1968). Conservatism in human information processing. In B. Kleinmuntz (ed.), *Formal Representation of Human Judgment*. New York: Wiley.

GELLER, E. S. & PITZ, G. F. (1968). Confidence and decision speed in the revision of opinion. *Org. Behav. hum. Perf.* **3**, 190–201.

HODGE, R. D. & LANZETTA, J. T. (1968). Effects of response uncertainty and degree of knowledge on subjective uncertainty. *Psychol. Rep.* **22**, 1081–1090.

PHILLIPS, L. & EDWARDS, W. (1966). Conservatism in a simple probability task. *J. exp. Psychol.* **72**, 346–354.

SANDERS, A. F. (1968). Choice among bets and revision of opinion. *Acta psychol.* **28**, 76–83.

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INFORMATION COMPLEXITY AND CROSS-MODAL FUNCTIONS

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Intra-modal and cross-modal presentations of a matching procedure were compared at two levels of information complexity in the same subjects. The modalities were visual and somatosensory. Sequence of information type and sex of experimenter and subject were systematically controlled. Pattern of accuracy among the several modality combinations and the effect of sequence varied as a function of information complexity. Sex of experimenter and subject yielded significant differences with simple information but not complex. The results, involving several detailed comparisons, confirmed generalizations derived from a review of prior studies in which only one type of information had been administered to any single group of subjects. Further examination of the relations between type of information and sensory modality is discussed.

In a recent review of research on sensory modality and information processing (Freides, 1974), it was suggested that informational complexity could account for many empirical and theoretical discrepancies in research on cross-modal relationships. More specifically, it appeared that if the information being processed was relatively simple, e.g. a unidimensional discrimination, cross-modal relations were strongly influenced by sensory parameters and contextual factors. If the information were more complex, e.g. pattern discrimination, contextual and parametric variables were of less importance, but the sensory modality was a critical variable, since each modality normally appeared to be particularly adept in dealing with certain kinds of information, and that received in another modality would be translated into the code of the adept modality for processing. Hence prediction of modal relationships would depend on the specific modality being stimulated and the kind of information – spatial, temporal or motor – being processed.

These conclusions were based on studies in which only one level of information complexity had been employed in any particular instance, hence there was no assurance that they were not based on sampling artifacts. This report presents cross-modal relations data on two levels of information complexity that were obtained from the same subjects. Following designs often used in this area, a matching problem was employed in which the standard and match could be in either the same or different modalities. The dependent measure was an index of matching accuracy which could be compared among the modal combinations. The unidimensional task – length estimation – was adapted from a procedure employed by Connolly & Jones (1970) in which visual and kinaesthetic modalities were compared, while for the complex information task – shape discrimination – visual and haptic modalities were compared. For convenience here, haptic and kinaesthetic will be termed somatosensory. With the first term designating the modality of the standard and the second the modality of the match, the modal combinations were visual – visual (VV), somatosensory–somatosensory (SS), visual–somatosensory (VS) or somatosensory–visual (SV).

On the basis of the generalizations in the review (Freides, 1974), cross-modal problems with simple information would be expected to be less accurate than their intra-modal counterparts (VV or SS > VS or SV) because there is less translation across channels, while SV would be more accurate than VS because (see Connolly & Jones, 1970 for details) information from the standard is translated to the output mode, and what is translated into a visual code will decay more slowly than what is translated into a somatosensory code. On the other hand, with pattern information of a spatial nature (forms), combinations with visual standards, whether intra-modal or cross-modal, should be more accurate than combinations with somatosensory standards (VV or VS > SV or SS), since vision is adept at processing (and hence retaining) spatial information and is the code to which the information is translated no matter in what sequence or modality received. From the same logic (credited to Pick, 1970, and elaborated by Freides, 1974) VS should be more accurate than SV. Finally, if simple information processing is more sensitive to contextual effects than complex information processing, sex of experimenter, sex of subject or their interaction might affect the results in the former case but not the latter. For this reason, these variables along with order of administration of the two types of task were systematically controlled in the design of the study.

METHOD

Design

Each subject was administered length and shape matching tasks intra-modally (VV and SS) and cross-modally (VS and SV). Modal combination order was randomized but administration of information type was systematically controlled along with sex of experimenter and of subject. Thus for each information type there were three between-subject variables, each with one degree of freedom, and one within-subject variable, modality combination, with three degrees of freedom.

Subjects

Forty male and forty female college students served as subjects. Eighty per cent were fulfilling a requirement in introductory psychology and the rest agreed to participate when asked. All were naive regarding the experiment.

Apparatus

An arena for presenting length and shape stimuli and for making matching responses was devised. It consisted of a box-like construction made of ¾ in. plywood covered with black vinyl contact paper in every visible area. It was positioned on a table at which the subject was seated. On the outside, a standing wall (61 cm^2) was seen, in the centre of which were an upper viewing aperture (15·5 cm high × 23 cm wide, beginning 10 cm below the top) and a lower arm aperture (15·5 cm high × 25·5 cm wide, originating at the floor of the arena). The experimenter could open or close the arena to view by manipulating a string which moved a shutter via a pulley. The arm aperture was draped with black felt to prevent visual access. Manual access was controlled by instruction.

Inside, the arena floor (44·5 cm wide × 30·5 cm deep) was bounded by the front wall and equally tall side walls. The one on the right originated at the edge of the front wall while the one on the left originated 14·5 cm in from the edge, thus leaving a concealed space behind the front wall for experimenter operations. Two ¾ in. plywood planks (vinyl covered, 44·5 cm wide × 10 cm deep) constituted stimulus platforms, one for shapes the other for lengths. When in use each was positioned on the floor of the arena, at the extreme rear.

Eight shape stimuli were copies of the second (set A here) and fourth (set B here) set of forms devised and illustrated by Lobb (1970, p. 351). Each consisted of one abstract form and three transformations that made the set similar yet different. They were cut out of black ⅛ in. plexiglass and mounted on 10 cm^2 pieces of ¼ in. plywood that had been covered with white shelving contact paper. The size of each form varied but they were all cut to the same scale. The base of the first figure in set B was 5·3 cm. Two similar shapes were devised and mounted for use only in practice trials. The centre of the 'shape' platform had three thin strips of black plexiglass mounted in a U configuration to guide the positioning of the stimuli, which were always presented in the same orientation.

Visual length stimuli consisted of ½ in. dowel rods covered with white contact shelving paper. Visual standards were 4, 6, 8, 10 and 12 in. long. They fitted over an 0·8 cm groove cut into the 'length' platform. They were always positioned with one end abutting the arena wall on the subject's left. For visual matching, a long dowel rod was inserted through a hole in the left wall just above the platform groove and it could be extended along the groove until a length match was made. Somatosensory length stimuli were presented by means of a stylus, a dowel rod, 25 cm long, tapered at one end with a 4 cm electrical tape grasping area beginning 11 cm from the tip. The stylus was moved in the groove from the wall on the left until a stop was encountered. These stops were other pieces of dowel rod cut so that when inserted in the groove at the right, 4, 6, 8, 10 or 12 in. was left free for traversal. When a somatosensory length match was being made the stops were removed. A ruler mounted at the back of the 'length' platform enabled the experimenter to read the discrepancy between standard and match.

Procedure

Subjects were assigned to conditions at random. The same sequence of trials was administered to each subject for each type of information. Before each trial the modality combination was announced with the appropriate sequence of the words 'touch' or 'look'. Four practice trials were given with each type of information. A 5 sec. delay separated the conclusion of presentation of the standard and the initiation of the match.

In the shape task one of the stimuli was presented first, and then either the same stimulus or another was presented. Judgements were called out by means of a number from 1–6 which, as stated in instructions and on a card constantly in view, signified both the decision, same or different, and the confidence level in three steps from guessing to assurance. Confidence level was employed only to increase concentration and involvement and was not analysed. For visual inspection the viewing aperture was lowered for 2 sec. Somatosensory shape exploration was for 4 sec. and was controlled by instructions to start and stop.

Ninety-six shape trials equally interspersed among the four modal combinations were administered. For each modal combination 12 same and 12 different stimulus pairs were presented. Stimuli and order of presentation were randomized except that the pair to be compared was always from the same set. From these data, the number of hits (when the judgement 'different' was given to different stimuli) and the number of false alarms (when the judgement 'different' was given to same stimuli) were calculated and a d' score was then determined by reference to published tables (Elliot, 1964). In addition, total correct (judgement 'different' to different stimuli plus judgement 'same' to same stimuli) was also calculated.

In the length task each subject matched each of the five dowel lengths in each of four modal combinations twice. Sequence of modal combination and dowel length varied randomly. Following the procedure of Connolly & Jones (1970), the discrepancy from the standard was recorded without regard to sign and scores for different lengths were combined. When the standard was visual, it was viewed for 2 sec. When the match was visual, the experimenter moved the long dowel rod down the groove until the subject indicated that the standard had been matched. Some backtracking was allowed. When the standard was somatosensory, the stylus was moved down the groove by the subject until a stop was encountered. This was done at the subject's own pace, but steady pacing was encouraged during practice.

RESULTS

Data for each information type were subjected to separate analyses of variance. All comparisons attaining statistical significance at $P < 0·05$ are reported below. There were no borderline results.

Length. Significant main effects occurred for sex of experimenter ($P < 0·001$), the female eliciting more accurate performance; sex of subject ($P < 0·05$), males being more accurate; order ($P < 0·025$), length analysis more accurate if performed first;

Table 1. *Mean scores for length and shape with corresponding accuracy ranks for each measure for each combination of visual* (*V*) *and somatosensory* (*S*) *modality*

	Length		Shape			
Modality	Matching deviation (inches)	Rank*	d'	Rank*	Total correct	Rank*
VV	0·42	1	3·26	1	21·97	1
SS	0·83	2	1·74	3	18·06†	3
VS	1·40	4	2·24	2	19·44	2
SV	1·19	3	1·38	4	17·30†	4

* Differences among all means are reliable at $P < 0·01$ by Newman–Keuls tests except where indicated.

† The difference between these means is reliable at $P < 0·05$.

and modal combination ($P < 0·001$). The means for the various combinations are presented in Table 1. The only reliably different interaction was between sex of experimenter and modal combination ($P < 0·01$). When the means were examined, it appeared that the result was because there was no experimenter difference in the VV modal combination, but the main effect recurred in all the others.

Shape. Significant main effects were found for order ($P < 0·025$), shape analysis being more accurate if performed second and for modality combination ($P < 0·001$). Means for the various combinations are presented in Table 1. Parallel findings were obtained with d' and total correct scores.

DISCUSSION

Generalizations gleaned from review of separate researches (Freides, 1974) were generally supported in the present study. When unidimensional information was being processed, intra-modal scores were more accurate than cross-modal and the cross-modal combination with a visual match was more accurate than the one with a visual standard (SV > VS). With more complex shape information, combinations with visual standards were more accurate than any other, whether intra-modal or cross-modal. This finding supports the concept of modality adeptness for complex processing. Sex of experimenter and subject (context variables) were significantly related to accuracy with simple but not with the more complex information. Since the same subjects generated both findings, this result seems specially striking. Finally, simple information was processed more effectively early in the session, perhaps because boredom became a problem later, while time in the situation facilitated the processing of more complex materials.

The main purpose of the present work was to bring together methods that had generated different patterns of results in order to determine whether those differences would persist if the same subjects were performing the two types of tasks. Before interpretation about different kinds of information processing can be assured, it would be helpful to provide closer control over a number of factors which might account for the results. These include closer identity in the modalities being compared for each kind of information (eliminating the haptic–kinaesthetic difference) and in the nature of task (same–different judgements and a signal detection procedure can readily be used with length). Further, it would be productive to study the effects of parametric variations on each type of information processing anticipating negligible effects in modality pattern with complex information and major effects with simple information. In any event, the present findings provide support for further consideration of information complexity in research on perception and information processing and for Garner's (1970) injunction to attend to the stimulus in information processing.

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REFERENCES

CONNOLLY, K. & JONES, B. (1970). A developmental study of afferent–reafferent integration. *Br. J. Psychol.* **61**, 259–266.

ELLIOT, P. B. (1964). Appendix 1. Tables of *d'*. In J. A. Swets (ed.), *Signal Detection and Recognition by Human Observers*. New York: Wiley.

FREIDES, D. (1974). Human information processing and sensory modality: cross-modal functions, information complexity, memory and deficit. *Psychol. Bull.* **81**, 284–310.

GARNER, W. R. (1970). The stimulus in information processing. *Am. Psychol.* **25**, 350–358.

LOBB, H. (1970). Asymmetrical transfer of form discrimination across sensory modalities in human adults. *J. exp. Psychol.* **86**, 350–354.

PICK, H. L. (1970). Systems of perceptual and perceptual motor development. In J. P. Hill (ed.), *Minnesota Symposia on Child Psychology*, vol. 4. Minneapolis: University of Minnesota Press.

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STIMULUS COMPLEXITY, EEG ABUNDANCE GRADIENTS, AND DETECTION EFFICIENCY IN A VISUAL RECOGNITION TASK

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Occipital EEG was monitored while subjects inspected 27 projected patterns. The number (N) and variety (V) of elements in the patterns were varied systematically. There were three levels of N (6, 12 or 24 elements) and three levels of V (circles, squares or hexagons occupying all, one half or one third of the element locations for all levels of N). Subjects were instructed that they would be required in a post-test to recognize the patterns, among patterns which had not appeared; they were also informed that the patterns had been constructed according to a set of simple rules, but the nature of these rules was not made fully explicit. The EEG was quantified by means of low-frequency analysis, yielding measures of abundance (theta, alpha and beta) and mean dominant frequency. For the recognition task, nine stimulus items were embedded among 45 items. Recognition efficiency was measured by means of the signal detection theory discrimination index (d'). The results were as follows: (i) Both N and V were inversely related to alpha abundance ($P < 0{\cdot}01$); (ii) the strongest relationship between stimulus parameters and the EEG held for N and EEG beta activity (13·5–19·5 Hz; $P < 0{\cdot}001$), where again the EEG and N were inversely related; (iii) there was a significant ($P < 0{\cdot}05$) direct relationship between N and theta activity; (iv) contrary to prediction, mean dominant alpha frequency decreased as N increased; (v) d' correlated significantly with a number of effects for N, i.e. subjects who exhibited greatest EEG discriminability of items during exposure of the patterns, subsequently obtained the higher detection scores in the recognition task. The work described therefore demonstrates that not only do stimulus parameters have systematic effects upon brain activity as measured by the EEG, but that such effects have functional value and reflect aspects of efficiency. The results are fully compatible with arousal theory constructs relating physiological reactivity and performance.

Several studies have demonstrated that stimulus complexity has systematic effects upon the human EEG, and the results obtained have been interpreted in terms of an arousal model (e.g. Berlyne & McDonnell, 1965; Gale *et al.*, 1971). A major difficulty in such work is the specification of the parameters of complexity, and most of the early work confounded these parameters. The only clear and unequivocal finding for visual stimuli is for *number of elements* in the display; for example, in a design which controlled for *variety*, *symmetry* and *ambiguity*, Gale *et al.* (1971) showed that alpha activity decreased linearly with $\log_2$ number of elements. This finding has been replicated in similar studies (Gale, Bramley *et al.*, 1972; Christie *et al.*, 1972). Christie *et al.* (1972) also varied *variety* while holding *number of elements* constant, but failed to obtain clear-cut results. While the stability of findings relating to *number of elements* is encouraging, it is clear that there are many other parameters which are of interest for studies of information-processing and experimental aesthetics. The first aim of the present study therefore is to manipulate both *number of elements* and *variety* in a design which allows for an assessment of both their independent and interacting effects.

Ratings obtained from subjects confirm that several parameters of complexity are experientially meaningful and induce variation in hedonic response (Berlyne & Peckham, 1966; Christie *et al.*, 1972). Christie *et al.* showed that both increased *number of elements* and *variety* were rated as more complex and that lower levels of *variety* were rated as more pleasing and beautiful than higher levels. Higher levels of visual stimuli which vary *number of elements* only are rated as more complex, interesting, beautiful and pleasant; and in the case of auditory stimulation, lower levels (i.e. repeated regular presentation of single tones) are rated as irritating and unpleasant (Gale, Bramley *et al.*, 1972). Such rating studies reveal a potential association between physiological variability and aesthetic judgement.

However, there is a fundamental and basic problem of experimental instructions for tasks of this nature. Subjects are required to do little more than gaze passively at stimuli for experimental periods exceeding 30 min. Such conditions of testing are hardly conducive to subject cooperation. Thus Berlyne *et al.* (1963) have shown that extrinsic motivation induced systematic variation in GSR response to their stimuli, whereas conditions lacking such motivation failed to do so. Instructions which give the task meaning for the subject are to be preferred and are most likely to yield systematic effects. The present study therefore employs instructional set to maximize subject participation.

Although variations in stimulus complexity have been shown to have systematic effects, there has been no demonstration of their *functional* value. The second aim of this study is therefore to examine the relationship between variations in the EEG during stimulus presentation and subsequent recognition efficiency.

EEG studies are often difficult to relate to each other because limitations of apparatus necessitate use of different measures of the EEG in different laboratories. Studies of stimulus complexity and the EEG are no exception; nevertheless, there is consistency among findings based upon visual analysis of *desynchronization* (Berlyne & McDonnell, 1965), automated averaging of *abundance* (Gale *et al.*, 1971) and *evoked potentials* (Nash & Fleming, 1970). The measure of *mean dominant alpha frequency* has proved useful in discriminating criterion groups based on individual differences in extraversion, imaging capacity and immediate recall (Gale *et al.*, 1969, 1974; Gale, Morris *et al.*, 1972). The final aim of this study is to measure the effects of *number of elements* and *variety* upon mean dominant alpha frequency.

METHOD

Subjects

The subjects were 20 male and female members of the Department of Applied Psychology subject pool, aged between 18 and 22 years. All subjects were school or higher education students, but none were students of psychology. Subjects were paid an attendance fee of 75p.

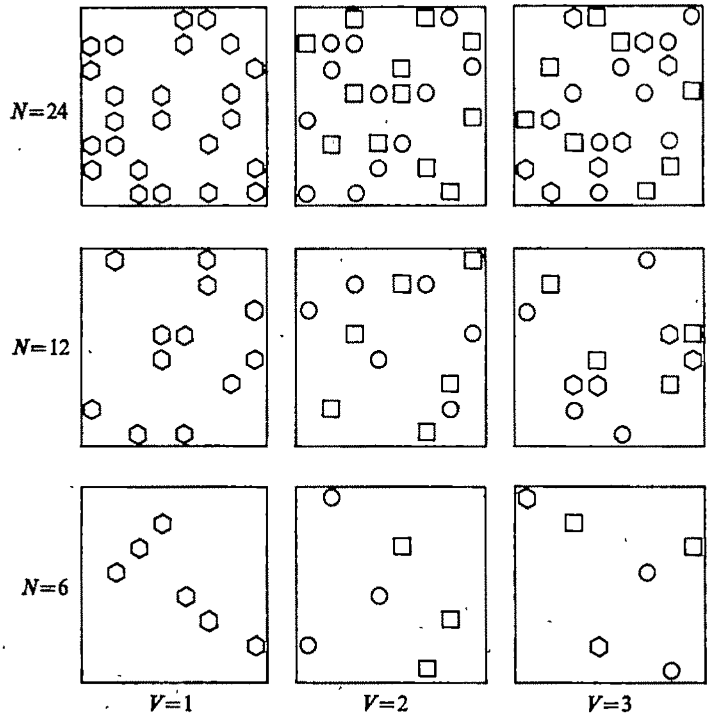


Fig. 1. The nine stimuli employed in the recognition task.

Recording of the EEG

The subject sat upright in a comfortable hairdresser's chair, viewing stimuli directly ahead. Bipolar recording was taken of transoccipital EEG by means of silver/silver-chloride loose pad electrodes, soaked in 5 per cent saline solution and retained by a rubber net. The reference electrode was placed directly above the mastoid. Interelectrode resistance was always below 10 k ohm. The primary recording was by means of a San'ei polygraph, calibrated at 30 mm/100 μV. (peak to trough), at a time constant of 0·1 sec. This unusually fast time constant was employed to minimize any slow drift in the record induced by eye movement. The primary record was then integrated by a San'ei Low Frequency Analyser (epoch time of 3 sec.) using nine pure bandpass filters set at the following frequencies: 4·5–6·5, 7·5–13·5 (in 6 × one-cyle steps), 8·0–13·0 and 13·5–19·5 Hz. The primary record was scanned for all these frequencies, for each epoch. The integrated outputs of the filters were converted by an analogue-to-digital converter into punched paper tape, which was then processed by computer.

Stimuli employed in the observation task

The stimuli were drawn in black ink on white card and then photographed, the photographic negative being made into slides. Thus the subject observed white stimuli against a dark grey background. The elements in the stimuli were placed within an invisible 8 × 8 matrix in vertical and horizontal locations determined by random numbers. Number of elements (N) were 6, 12 or 24 per slide. Variety (V) was represented by white contours of squares, circles or hexagons, all of uniform area. There were three sets of nine slides, each containing three levels of N and V. Thus each slide had either 6, 12 or 24 elements, which were wholly one level of V (e.g. circles) or equally divided between two levels, e.g. $N = 12$, $V = 2$ (6 squares plus 6 circles), or three levels, e.g. $N = 12$, $V = 3$ (4 squares plus 4 circles plus 4 hexagons). Thus both N and V were manipulated systematically within one design (see Fig. 1). Each subject was allocated his or her own random schedule, randomized both for blocks of nine and for items within blocks. The only constraint on the schedules was that the nine items selected for the recognition task (see below) were distributed evenly over the observation task (i.e. three recognition items per block of nine slides). The stimulus was projected over the subject's head, into an experimental cubicle which was illuminated only by the projector beam. The projected image was 80 cm square and the distance between the subject and the screen was 180 cm. Each slide was presented for 30 sec., giving a total task time of 13·5 min. The slide projector was operated automatically by the EEG analyser, which stepped on the projector after 10 3 sec. epochs. Thus EEG sampling and stimulus exposure were synchronized.

Instructions for the observation task

One slide was selected at random and presented as a test item. The subject was then presented with the following instructions on a printed sheet. Each key aspect of the instructions was numbered, but the numbers have been omitted here. 'You are going to see a set of patterns, one at a time, similar to the one on the screen. Please look at the *whole* pattern. Do not look at just part of it. The patterns have been constructed from simple shapes according to a set of simple rules. The rules determine the ***number*** and ***variety*** of shapes in the pattern. The way the shapes are arranged is of ***no importance or significance***. Do not attend to it. You will be asked to identify the patterns after you have seen them. They will be shown to you among a set of patterns which did ***not*** appear, and you will be asked to identify the ones which did appear. We are recording your brain waves. Our apparatus is very sensitive, so please sit still, but in a relaxed manner. Don't tense up, don't move and please keep your eyes open throughout the experiment. Keep your mind on the experiment. You will have plenty of time to observe each pattern. There is no rush. Sufficient examples will be shown to help you to work out the principles underlying the construction of the patterns. Remember, you will be asked afterwards to identify the patterns which you have seen. Please ask *any* questions you wish about the experiment now. Read these instructions again to make sure you have understood them.' The above instructions were designed to establish rapport and to remove any ambiguity about the purpose of the task. They emphasize the problem-solving and memory aspects of the task.

The recognition task

A complete set of nine true stimuli were combined with 36 false stimuli. Each stimulus pattern (9 cm square) was printed separately on a sheet of white paper. Each subject was given a set of 45 sheets, with a different random presentation for each subject. Since it was likely that subjects would grasp the principle underlying the V factor without difficulty, and therefore obtain perfect scores for any false items containing new V elements, no new V elements were introduced. The false stimuli in the recognition task were varied for N but this of course necessitated disruption of the ratios of V elements. There were four false stimuli per true stimulus, varied systematically as ratios of N of the true stimuli. Thus for $N = 24$ (true stimulus), false stimuli were 16, 20, 28 and 32 in number, giving the ratios of $(4/6)\ N$, $(5/6)\ N$, $(7/6)\ N$ and $(8/6)\ N$.

The false stimuli were derived from true stimuli by removing or adding elements. This precaution was designed to ensure that subjects would be less likely to employ stimulus configuration as a cue to recognition.

No indication was given of the total number of true stimuli to be recognized or the manner in which false stimuli varied. The following printed instructions were read by the subject. 'Here are a series of patterns. Some of these patterns have appeared on the screen. Look at each one in turn. If you recognize the pattern as one you have seen, please tick it. Place a cross beside those patterns which you think you have not seen. Once you have ticked or crossed a pattern, please turn it over and place it face down, *before* you look at the next one. Please do not rush this part of the experiment. Make your decisions as carefully as possible.'

RESULTS

Number of elements, variety and the EEG (abundance)

Each subject saw 27 slides, with three equivalent sets of nine slides. There were 10 3 sec. analyser epochs associated with each slide. These values were collapsed to yield six summed scores for each subject: one for each level of N and one for each

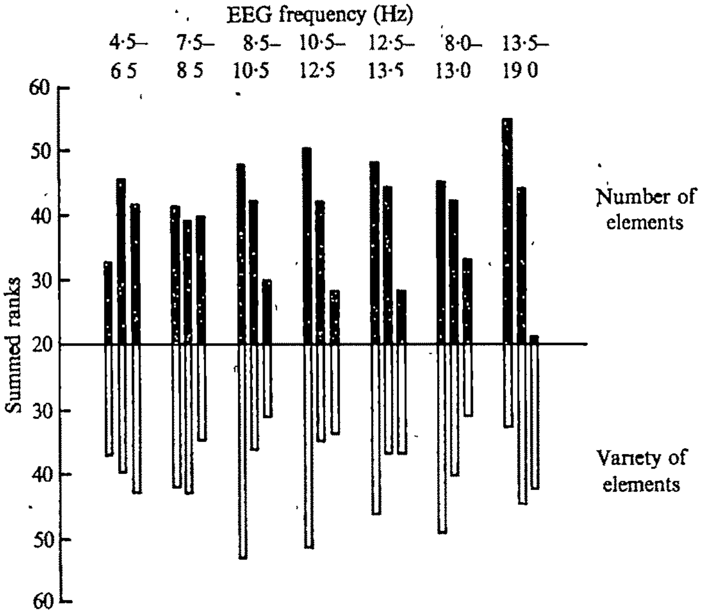


Fig. 2. Summed ranks of EEG abundance values for each of the frequency bands. In each case, increasing values of N (6, 12, 24) and V (1, 2, 3) are from left to right.

Table 1. *Mean integrated abundance values for N and V, together with the results of Friedman analyses of variance, and Wilcoxon matched-pairs, signed-rank tests, employed for multiple comparisons.*

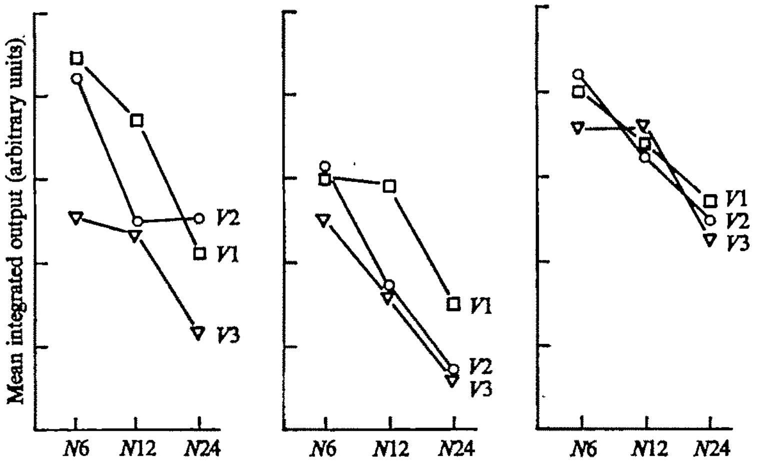
(The mean values are for 30 sec. (10 × 3 sec. epochs) over nine slides ($N = 20$). Significance levels for the Friedman tests are two-tailed. Since the Wilcoxon tests were used to compare scores whose direction of difference was already established, a one-tailed criterion of significance was adopted.)

(*a*) Number of elements (N)

EEG (Hz)	Mean integrated output			Friedman	Wilcoxon		
	N = 6	N = 12	N = 24		6 > 12	6 > 24	12 > 24
4·5/6·5 (theta)	284·88	287·61	286·71	n.s.	$P <$ 0·04	n.s.	n.s.
8·0/13·0 (gross alpha)	238·07	235·85	233·79	n.s.	n.s.	n.s.	n.s.
7·5/8·5 (low alpha)	186·74	186·55	183·12	n.s.	n.s.	n.s.	n.s.
8·5/10·5 (mid alpha)	218·56	214·41	209·51	$P <$ 0·01	$P <$ 0·004	$P <$ 0·005	$P <$ 0·05
10·5/12·5 (high alpha)	214·24	209·95	205·40	$P <$ 0·01	$P <$ 0·05	$P <$ 0·01	$P <$ 0·01
12·5/13·5 (very high alpha)	210·90	206·81	200·15	$P <$ 0·01	$P <$ 0·001	n.s.	$P <$ 0·04
13·5/19·5 (beta)	139·61	137·28	132·15	$P <$ 0·001	$P <$ 0·005	$P <$ 0·005	$P <$ 0·005

(*b*) Level of variety (V)

EEG (Hz)	Mean integrated output			Friedman	Wilcoxon		
	$V = 1$	$V = 2$	$V = 3$		1 > 2	1 > 3	2 > 3
4·5/6·5 (theta)	286·20	286·52	286·49	n.s.	n.s.	n.s.	n.s.
8·0/13·0 (gross alpha)	237·88	235·96	233·87	$P <$ 0·01	$P <$ 0·04	$P <$ 0·01	n.s.
7·5/8·5 (low alpha)	187·89	185·31	183·21	n.s.	n.s.	n.s.	n.s.
8·5/10·5 (mid alpha)	217·08	215·45	209·95	$P <$ 0·01	$P <$ 0·01	$P <$ 0·01	n.s.
10·5/12·5 (high alpha)	212·38	208·80	208·41	$P <$ 0·02	$P <$ 0·01	$P <$ 0·05	n.s.
12·5/13·5 (very high alpha)	207·84	205·21	204·81	n.s.	n.s.	n.s.	n.s.
13·5/19·5 (beta)	136·92	136·58	135·54	n.s.	n.s.	n.s.	n.s.



Mid alpha
8·5–10·5 Hz

High alpha
10·5–12·5 Hz

Beta
13·5–19·5 Hz

Fig. 3. Abundance values for each cell represented in Fig. 1.
Each point plotted is the mean of three slides for 20 subjects.

Table 2. *Results of Wilcoxon matched-pairs, signed-ranks tests upon data represented in Fig.* 3

Level of number of elements	Levels of variety								
	8·5–10·5 Hz			10·5–12·5 Hz			13·5–19·5 Hz		
	1 > 3	1 > 2	2 > 3	1 > 3	1 > 2	2 > 3	1 > 3	1 > 2	2 > 3
6	$P < 0·001$	$P < 0·005$	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
12	$P < 0·05$	$P < 0·005$	n.s.	$P < 0·05$	$P < 0·005$	n.s.	n.s.	n.s.	n.s.
24	n.s.	n.s.	$P < 0·01$	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.

level of V. Values for N and V were then treated separately for the purposes of statistical analysis. Non-parametric statistics were employed; Friedman two-way analyses of variance (Siegel, 1956) to test for ranking of the three levels, and Wilcoxon matched-pairs signed-ranks to test for differences between pairs of conditions. These analyses were conducted upon the following frequency ranges: 4·5–6·5, 7·5–8·5, 8·5–10·5, 10·5–12·5, 12·5–13·5, 8·0–13·0 and 13·5–19·5 Hz. The values of the summed ranks for N and V are illustrated in Fig. 2; group means and results of the statistical analyses are shown in Table 1.

For the three EEG frequencies which yielded the stronger results (8·5–10·5, 10·5–12·5, and 13·5–19·5 Hz) further comparisons were made for V at each level of N. The results of these analyses are shown in Fig. 3 and Table 2.

Number of elements, variety and the EEG (mean dominant alpha frequency)

Mean dominant alpha frequency (m.d.f.) was calculated for N and V separately, for each subject, employing the six values obtained from the six filters falling between 7·5 and 13·5 Hz, by dividing the sum of products of central filter values and their integrated outputs by the sum of integrated outputs. This yielded three

Table 3. *Rank correlations between various measures of EEG abundance and the d' score for recognition efficiency*

	8·5/10·5 Hz	10·5/12 Hz	12·5/13·5 Hz	13·5/19·5 Hz
(i) EEG level 6 minus Level 24	+0·07	+0·42*	0·00	+0·27
(ii) EEG Ratio Level 6: Level 24	+0·09	+0·45*	+0·01	+0·43*
(iii) Slope Levels 6: 12: 24	−0·06	+0·49*	−0·03	+0·11
(iv) Goodness of fit to linearity	−0·17	+0·39*	+0·23	+0·03
(v) (i) and (ii)	+0·96**	+0·97**	+0·89**	+0·69**

* $P < 0{\cdot}05$ (critical value is 0·37). ** $P < 0{\cdot}01$.

m.d.f. values for N and for V. M.d.f. decreased as N increased, i.e. the values were: $N = 6$, m.d.f. 10·562; $N = 12$, m.d.f. 10·536; and $N = 24$, m.d.f. 10·531. Only the difference between $N = 6$ and 12 was significant ($P < 0{\cdot}04$; Wilcoxon matched-pairs, signed ranks). For V, the m.d.f. values for 1, 2 and 3 levels were 10·547, 10·537 and 10·546 respectively; there were no significant differences between these values.

Detection efficiency and EEG discrimination of stimuli

Since the key factor varied in the recognition task was N, only those EEG measures relating to N were related to recognition performance. Efficiency of recognition was assessed by calculating d' for each subject, according to the formula given by Welford (1968), i.e. including measures of hits, correct rejections, misses and false positives. The d' scores ranged between 0·30 and 4·21 with a mean of 1·56. Correct detections ranged between 2 and 9 (the maximum) with a mean of 6·3. (An informal analysis of the nature of errors showed that these were all related to N and increased the closer the false stimulus was for N to the true stimulus.) Taking the four frequency bands which gave the strongest results in Table 1, Spearman rank correlation coefficients (Siegel, 1956) were computed with d' for the following indices of EEG discrimination of stimuli: (i) absolute difference of abundance between $N = 6$ and $N = 24$, (ii) ratio of abundance values for $N = 6$ and $N = 24$, (iii) slope for $N = 6$, and 24, and (iv) goodness to fit to linearity of (iii). The results of these analyses are shown in Table 3. Rank correlations were computed between (i) and (ii) and the high values obtained (see Table 3) indicate that the different values associated with $N = 6$ and 24 are unlikely to be confounded with base level effects i.e. dependence of degree of change upon initial level.

DISCUSSION

There are four key results in this study. (i) As in earlier work, EEG abundance is inversely related to number of elements (N); (ii) EEG abundance is inversely related to variety (V); (iii) for the stimuli employed there are no striking interaction effects between N and V; (iv) individual differences for detection efficiency in visual recognition relate to degree of differential EEG response during exposure to stimuli. Finally, contrary to prediction, mean dominant alpha frequency is inversely related to N.

The findings for number of elements (N) replicate the earlier studies referred to in

P 4811

the introduction and are compatible with arousal theory notions concerning the influence of differential arousal value of stimuli upon the organism (e.g. Berlyne, 1960). However, in this case instructional set has shifted some of the effects to the upper tail of the recorded EEG range. Previous studies reporting measures of beta abundance (13·5–19·5 Hz) have obtained only negligible effects. The very modest effects for our broad-band alpha filter (8·0–13·0 Hz) indicate how effects may be overlooked when narrow-band analysis is omitted; previous studies employing broad band-widths for alpha measurement may have failed to detect genuine effects. In certain circumstances the total energy output for gross alpha may remain constant in spite of systematic variation within a narrow frequency band, or in spite of reciprocal variation at the two tails of the alpha spectrum. Variety (*V*) affects gross alpha and in particular the mid-alpha range, rather than the upper alpha and beta ranges. Conditions of testing make the *N* variable more complex than *V*. The subject may grasp the principle underlying manipulation of *V* without great difficulty, since it only involves recognition that there are one, two or three types, dividing the stimulus elements into equal proportions. In the case of *N*, however, detailed inspection of the stimulus, probably involving counting of elements, is required. Although little is known concerning the functional significance of variation within different alpha or extra-alpha frequencies, we would like to suggest that more complex tasks are likely to have maximal effects upon the upper alpha frequencies. 'Task' in this case would be defined in terms of stimulus characteristics and instructional demands made upon subjects.

The m.d.f. finding for *N* is contrary to prediction, since it is inconsistent with the abundance measures, higher frequencies being normally associated with lower amplitude (e.g. Gale *et al.*, 1969). The finding that lower amplitudes are associated with lower frequency is unusual but not unique: Broadhurst & Glass (1969) obtain similar results in a study of EEG correlates of extraversion and neuroticism. This is the first study to obtain an m.d.f. measure in relation to stimulus complexity and further replication is required. It should be noted that the abundance scores upon which the m.d.f. calculations are based confound frequency with prevalence. Thus one possible source for the indirect relationship between abundance and m.d.f. findings could be that lower frequencies occurred with higher amplitudes but were present for less of the time than higher frequencies. It may be noted that increased m.d.f. with increased complexity would have been compatible not only with our predictions based upon arousal theory but also with the oculomotor hypothesis concerning the origin of the alpha rhythm (Lippold, 1970). We may derive comfort from the observation that our finding for m.d.f. is incompatible with Lippold's (1970) theory, which would predict increased alpha frequency with the increased rates of visual scanning called for by more complex stimuli.

The increase of theta activity with stimulus complexity or increased arousal is expected on the basis of previous findings. Gale *et al.* (1971), Daniel (1967) and Haslum & Gale (1973) all show increased theta abundance at higher levels of task induced arousal. Such findings may reflect hippocampal theta activity which is known to have a reciprocal relationship with alpha (Jouvet, 1961).

We have observed no systematic interaction between *N* and *V*. At very high levels of *N*, *V* may have little effect on the EEG (e.g. Christie *et al.*, 1972). Consideration of such problems requires a richer range of the stimuli than those used in the present experiment.

Considerations of experimental load upon the subjects precluded inclusion of rating scales on this occasion. In future studies we propose to vary *N* and *V* in a pilot procedure to obtain subjective estimates of decline of *N* effects at different levels of *V* and vice versa, as a prelude to further EEG observations, since such judgements might enable us to select the appropriate stimulus ranges for *N* and *V* before engaging in lengthy experimentation.

Our most novel finding is that measures of EEG discrimination during inspection of the patterns relate to subsequent capacity to recognize the patterns. The more stimuli are differentiated in terms of EEG abundance, the more efficient is subsequent performance. Thus EEG changes which accompany variation in stimulus complexity are shown to have functional significance. We believe that the discovery of such a discrete relationship between CNS activity during stimulus scanning and subsequent performance is unique. Haslum & Gale (1973) have shown that degree of differential EEG response to the cue value of a variety of wanted and unwanted stimuli in a vigilance task relates to speed of reaction time to wanted signals. There appears to be evidence therefore for the view that capacity for differential physiological response to the various stimuli included in a task relates to aspects of competence. Thus a comparatively simple measure of CNS activity may be related to a quite complex composite signal detection measure.

Ashby (1952) emphasizes the importance of biological variability potential, for efficiency in general processes of adaptation to environmental change. Lindsley's original statement of activation theory (1960) argued for a linear relationship between EEG activation and efficiency yielding a range of attentive states matching levels of awareness and behavioural competence; however his theory lacked empirical support for such variation within the waking state. The present abundance findings provide support for activation theory and for the view that variability as such is of importance, since more efficient subjects exhibit a greater range of physiological differentiation.

A final problem concerns the nature and origin of this relationship. Does the correlation between EEG characteristics and performance reveal a causal relationship? Are such physiological activities antecedent to behavioural or experiential events or merely parallel in time? In view of our recent findings concerning EEG resting characteristics and subsequent performance in short-term recall (Gale *et al.*, 1974), we are inclined to favour the causal view. However, any speculation concerning the relationship between EEG characteristics and experiential strategies employed in the present task, would oblige us to go well beyond the observed data.

Psychophysiologists are fully aware of the pitfalls of the Cartesian cul-de-sac, but such awareness does not necessarily imply a capacity to resist its temptations.

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REFERENCES

ASHBY, W. R. (1952). *Design for a Brain*. London: Chapman & Hall.

BERLYNE, D. E. (1960). *Conflict, Arousal and Curiosity*. London: McGraw-Hill.

BERLYNE, D. E., CRAW, M. A., SALAPATEK, P. H. & LEWIS, J. L. (1963). Novelty, complexity, incongruity, extrinsic motivation and the GSR. *J. exp. Psychol.* **66**, 560–567.

BERLYNE, D. E. & MCDONNELL, P. (1965). Effects of stimulus complexity and incongruity on duration of EEG desynchronization. *Electroenceph. clin. Neurophysiol.* **18**, 156–161.

BERLYNE, D. E. & PECKHAM, S. (1966). The semantic differential and other measures of reaction to visual complexity. *Can. J. Psychol.* **20**, 125–135.

BROADHURST, A. & GLASS, A. (1969). Relationship of personality measures to the alpha rhythm of the electroencephalogram. *Br. J. Psychiat.* **115**, 199–204.

CHRISTIE, B., DELAFIELD, G., LUCAS, B., WINWOOD, M. & GALE, A. (1972). Stimulus complexity and the EEG: differential effects of the number and the variety of display elements. *Can. J. Psychol.* **26**, 155–170.

DANIEL, R. S. (1967). Alpha and theta EEG in vigilance. *Percept. mot. Skills* **25**, 697–703.

GALE, A., BRAMLEY, P., CHRISTIE, B. & LUCAS, B. (1972). Differential effect of visual and auditory complexity on the EEG: negative hedonic value as a crucial variable? *Psychon. Sci.* **17**, 21–24.

GALE, A., CHRISTIE, B. & PENFOLD, V. (1971). Stimulus complexity and the occipital EEG. *Br. J. Psychol.* **62**, 527–531.

GALE, A., COLES, M. & BLAYDON, J. (1969). Extraversion–introversion and the EEG. *Br. J. Psychol.* **60**, 209–223.

GALE, A., JONES, D. M. & SMALLBONE, A. (1974). Short-term memory and the EEG. *Nature, Lond.* **248**, 439–440.

GALE, A., MORRIS, P. E., LUCAS, B. & RICHARDSON, A. (1972). Types of imagery and imagery types: an EEG study. *Br. J. Psychol.* **63**, 523–531.

HASLUM, M. N. & GALE, A. (1973). Inter-modal and intra-subject consistency in EEG correlates of vigilance. *Biol. Psychol.* **1**, 139–150.

JOUVET, M. (1961). Telencephalic and rhombencephalic sleep in the cat. In G. E. W. Wolstenholme & M. O'Connor (eds.), *CIBA Symposium on the Nature of Sleep*. London: Churchill.

LINDSLEY, D. B. (1960). Attention, consciousness, sleep and wakefulness. In J. Field, H. W. Magoun & V. E. Hall (eds.), *Handbook of Physiology*, vol. 3, sect. 1. Washington, D.C.: American Physiological Society.

LIPPOLD, O. C. J. (1970). The origin of the alpha rhythm. *Nature, Lond.* **226**, 616–618.

NASH, M. D. & FLEMING, D. E. (1970). The dimensions of stimulus complexity and the visually evoked potential. *Neuropsychol.* **8**, 171–177.

SIEGEL, S. (1956). *Nonparametric Statistics for the Behavioural Sciences*. New York: McGraw-Hill.

WELFORD, A. T. (1968). *Fundamentals of Skill*. London: Methuen.

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ANXIETY AND RITUALIZED SPEECH

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The experiment examines the effects on a number of words that seem irrevelant to semantic communication. The Units of Ritualized Speech (URSs) considered are: 'I mean', 'in fact', 'really', 'sort of', 'well' and 'you know'. Two hypotheses are tested: (i) that URS rate will increase with anxiety; and (ii) that the speaker's preferred URS will increase with anxiety. Subjects were interviewed on topics they had previously rated as anxiety-provoking and non-anxiety-provoking. Hypothesis (i) was supported, but hypothesis (ii) was not. More specifically, the use of 'I mean' and 'well' increases when the speaker is anxious. Explanation for this is sought in the grammatical location of these two units. Sex differences in the use of URSs, correlations between URSs and their relationship to other forms of disfluency are also considered.

The study of interruptions in the continuity of grammatical speech has a long history. Freud (1914) explored the dynamics of such phenomena as slips of the tongue and concluded that psychological conflict was an important precondition for their occurrence. Baker (1948) considered 'hesitatory formulae' such as 'I don't know', 'I mean 'and 'Well', together with other speech disturbances like repetitions and uncompleted sentences, and argued that they should all be included in the concept of paraphasia. He analysed 29 hours of verbatim record of the psychoanalytic treatment of a psychopath and concluded that these disfluencies are the result of repressed ideas in the same way as name forgetting, slips of the tongue and other cases of *lapsus linguae* that Freud studied. Baker maintains that there is a close relation between these and the speaker's anxieties. Feldman (1948, 1959) claims that such disfluencies are used by the speaker as an aid to overcome momentary difficulties in speaking. Early work in the area was largely anecdotal or speculative, and systematic study of interruptions in the continuity of normal speech was initiated in 1956. Mahl (1956) put forward an inductively developed classification of disfluency. The categories he distinguishes are as follows: 'ah', sentence change, repetition, stutter, omission, sentence incompletion, tongue-slip and intruding incoherent sound. (For full details of the system, see Mahl, 1956; Kasl & Mahl, 1965.) The 'non-ah' speech disturbances (i.e. all speech disturbances other than 'ah') have been shown to increase when subjects are made anxious (Kasl & Mahl, 1965; Siegman & Pope, 1965; Cook, 1969; Feldstein *et al.*, 1963). The relation of the 'ahs' (or filled pauses) to anxiety is less clear. There seems to be an important sex difference, both in the rate of occurrence of filled pauses, and in their relation to anxiety. Feldstein *et al.* (1963) and Lalljee & Cook (1973) have shown that men have a higher filled pause ratio than women, and it seems possible that there is an increase in filled pauses with anxiety for men but not for women.

Most of the research in the area has been conducted using Mahl's categories, but some other forms of disturbance have also been studied. Dibner (1956) distinguishes two Speech Cue Scales. Cue Scale 1 is very like Mahl's non-ah ratio, but Cue Scale 2 includes instances of laughing, voice change and sighing or deep breath. It seems likely that Cue Scale 2 is also related to anxiety (Krause & Pilisuk, 1961).

This paper is concerned with the relationship to anxiety of several familiar interruptions of continuity. The speech units that will be considered are: 'I mean', 'in fact', 'really', 'sort of', 'well' and 'you know'. These speech units occur in a ritualized manner and seem irrelevant to the mainstream of the semantic communication. They will be referred to as Units of Ritualized Speech (URSs). The choice of a particular URS is probably a function of the speaker's subculture and also of the historical period. Those that have just been listed were chosen because they were felt to be frequently used within the student subculture from which the subjects for the experiment came. Obviously the words and phrases in question are sometimes used in a way that is not 'ritualized' (e.g. '...as well' or 'Do you know...'), but this exploratory study does not attempt to distinguish between the different usages of these units.

Anxiety is known to result in an increase in interruptions in the continuity of speech and it is hypothesized that URSs will increase when the speaker is anxious.

A subsidiary hypothesis is also put forward. It was reported earlier that men have a higher filled pause ratio than women, and that it seems possible that filled pauses increase with anxiety for men but not for women. It also seems likely that the use of a particular URS may be to some extent idiosyncratic (Bernstein, 1962). In order to take into account individual differences in the use of URSs it is hypothesized that the URS that a person uses most in non-anxious speech (i.e. his 'preferred URS') will increase significantly with anxiety.

Sex differences in the use of URSs will also be explored, as well as their frequency of occurrence and their correlations with each other and with other indices of verbal fluency.

METHOD

Data from a previous experiment on the effects of anxiety on speech (Cook, 1969) were used for this study. The subjects were 10 students, five males and five females. One of the experimenters (M.C.) interacted with each subject for about half an hour. Before the interaction, each subject filled out a questionnaire which asked the subjects about whether or not they were anxious about a series of topics. The interaction was divided into three periods of approximately 10 min. each. During the first 10 min. (period 1) and the last 10 min. (period 3) of the interaction, the experimenter chose topics of conversion that the subjects had rated as not being sources of anxiety. During the middle period (period 2) the experimenter chose topics that the subjects had rated as anxiety-provoking. The experimenter interacted with the subjects in a naturalistic way, commenting on what was said and also discussing his own experiences. Each interaction was recorded on a Ferrograph tape-recorder and the recordings were transcribed. (For full details of the method see Cook, 1969.)

RESULTS

For the present study, non-ah ratios were used as an index of anxiety. These are higher in period 2 than in period 1, but there was no significant difference between periods 2 and 3. Cook (1969) argues that the anxiety generated in period 2 took time to dissipate. Period 3 will therefore be ignored in the following discussion. One subject who showed no increase in non-ah ratio in period 2, the 'anxious period', was excluded because the anxiety induction does not seem to have been

Table 1. *Summary of analysis of variance*

Source	D.F.	General URS ratio	Preferred URS ratio	*F* ratios: I mean	In fact	Really	Sort of	Well	You know
Between subjects	8								
Sex	1	1·46	1·03	6·60*	< 1·00	2·08	< 1·00	< 1·00	5·18
Subjects within groups	7								
Within subjects	9								
Period	1	10·54*	< 1·00	7·39*	2·49	1·17	< 1·00	6·79*	< 1·00
Period × sex	1	< 1·00	2·73	3·64	4·37	1·32	1·90	< 1·00	< 1·00
Period × subjects within groups	7								

* $P < 0·05$.

Table 2. *Means and standard deviations for general URS ratio*

(In number of URSs per 1000 words.)

Period effect*	Non-anxious period	Anxious period
Mean	25·98	36·01
S.D.	7·5	10·8
Sex effect†	**Females**	**Males**
Mean	34·53	28·13
S.D.	2·3	10·4

* The difference between the two periods is significant at the 5 per cent level.
† The difference is not significant.

successful in that case. Lalljee & Cook (1973) have shown that hesitation rates in the first few minutes of an interaction are typical, and therefore the first 3 min. of each interaction are also excluded. The results presented below compare the first (i.e. non-anxious) period excluding the first 3 min., with the second (i.e. anxious) period.

URS ratios were calculated for each subject by summing the number of URSs for each period of the interaction and dividing by the number of words spoken in that period. Similarly, ratios were calculated for each URS separately.

A two-way analysis of variance (sex × period) for repeated measures was calculated for the general URS ratio. The results are presented in Table 1. The effect of period is significant. There is a significant increase in URSs when the speaker is anxious. Table 2 presents the means and standard deviations for the main effects.

A two-way analysis of variance (sex × period) for repeated measures was calculated for the preferred URS ratio. The results are presented in Table 1. Neither the main effects nor the interaction is significant. Means and standard deviations of the main effects are presented in Table 3.

Additional findings

Analysis of each URS. Separate analysis of variance were calculated for each URS in order to test the significance of the difference for each URS in the two experimental periods. Table 1 reports the results of the analyses of variance. Table 4

Table 3. *Means and standard deviations for preferred URS ratio*

(In number per 1000 words.)

Period effect*	Non-anxious period	Anxious period
Mean	10·13	12·02
S.D.	4·1	11·5
Sex effect*	**Females**	**Males**
Mean	8·34	13·27
S.D.	2·9	9·2

* The difference is not significant.

Table 4. *Means and standard deviations for each URS*

(In number per 1000 words.)

Sex and experimental condition.

			Non-anxious	Anxious
I mean*†	Males	Mean	0·90	1·83
		S.D.	0·9	2·6
	Females	Mean	2·44	6·88
		S.D.	1·9	3·6
In fact	Males	Mean	2·68	0·80
		S.D.	0·9	1·2
	Females	Mean	2·38	2·80
		S.D.	3·1	1·7
Really	Males	Mean	1·43	5·20
		S.D.	0·9	6·1
	Females	Mean	6·02	5·66
		S.D.	3·5	1·7
Sort of	Males	Mean	3·74	6·34
		S.D.	2·7	5·5
	Females	Mean	5·10	2·82
		S.D.	4·2	2·6
Well*	Males	Mean	9·41	16·81
		S.D.	6·3	13·2
	Females	Mean	8·69	12·58
		S.D.	2·2	3·1
You know	Males	Mean	3·65	3·40
		S.D.	2·6	2·4
	Females	Mean	6·40	6·60
		S.D.	3·4	5·3

* Non-anxious *v.* anxious, $P < 0{\cdot}05$.
† Males *v.* females, $P < 0{\cdot}05$.

Table 5. *Location of URSs within sentences*

URS	Beginning	Middle	End	Total no. of occurrences
I mean	54 +	43 –	3 0	100
In fact	11 0	33 –	20 +	64
Really	0 – –	102 0	17 0	119
Sort of	6 –	160 + +	6 –	172
Well	214 + +	70 – –	20 0	304
You know	24 0	82 –	40 +	146

0, no significant difference between observed and expected frequency.
+, + +, observed frequency is greater than expected frequency (+, $P < 0{\cdot}05$; + +, $P < 0{\cdot}01$).
–, – –, observed frequency is smaller than expected frequency (–, $P < 0{\cdot}05$: – –, $P < 0{\cdot}01$).

presents the means and standard deviations for each URS in each period for each sex.

The occurrence of 'Well' and of 'I mean' increases during the anxiety period. Women use 'I mean' more frequently than men. The sex difference in the use of 'you know' (women using 'you know' more frequently than men) does not reach the 5 per cent level of confidence.

Grammatical analysis. A grammatical analysis was performed in order to investigate whether or not the URSs studied occur at different parts of the sentence. Full stops were included in the transcripts by an experienced secretary who listened to the tape-recordings while marking the transcripts. These were used as criteria for the ends of sentences. The entire corpus of speech collected was used for this analysis.

Three locations within the sentence were distinguished: (1) before the first word; (2) middle (i.e. after the first word but before the last word); and (3) after the last word. The observed frequencies of occurrence of URSs in the three grammatical locations are presented in Table 5. These observed frequencies were then tested against the frequencies expected by chance. Every URS was considered separately. For each subject the expected probability of occurrence in each of the three categories was based on the mean sentence length of that subject. The significance of the number of subjects who produced more (or less) of a particular URS in a particular location was evaluated by means of the Wilcoxon matched-pairs signed-ranks test (see Siegel, 1956). Table 5 presents a summary of these results.

The results support the notion that the URSs studied distribute differently. 'Well' and 'I mean' occur at the beginnings of sentences more frequently than can be expected by chance. 'Sort of' occurs predominantly in the middle of the sentence and 'you know' and 'in fact' predominantly at the end.

Correlations. Correlations between subjects for the URSs as well as for speech rate, filled pauses and non-ah speech disturbances, were calculated. The entire corpus of speech collected was used for this analysis. The results are presented in Table 6. There is a strong positive correlation between speech rate and 'you know', 'sort of', 'really' and 'in fact'. 'Well' and 'I mean' are negatively correlated, but 'you know' is positively correlated with 'in fact' and 'really' with 'sort of'.

Table 6. *Correlation between subjects*

	Non–ah ratio	Filled pauses	Well	I mean	You know	Sort of	Really	In fact
Filled pauses	0·43							
Well	−0·34	−0·26						
I mean	0·02	−0·54	−0·45					
You know	0·00	−0·18	−0·25	0·53				
Sort of	0·32	−0·08	0·06	0·15	0·42			
Really	0·23	−0·42	−0·09	0·46	0·31	0·65*		
In fact	−0·52	−0·31	−0·06	0·24	0·63	0·21	0·00	
Speech rate	−0·12	−0·38	0·09	0·40	0·60	0·71*	0·57	0·65*

* $P < 0{\cdot}05$.

Discussion

The major hypothesis, that URSs increase when a person is talking about an anxiety-provoking topic, is confirmed. No support was obtained for the subsidiary hypothesis that the preferred URS increases with anxiety. However, the separate analyses for each URS show that only 'well' and 'I mean' increase significantly with anxiety.

It can be argued that the results could be accounted for by postulating a general increase in the rate of URSs with time. Time series effects have been examined by Lalljee & Cook (1973). They found no general increase in hesitation rates with time. In fact, filled pause ratio decreases and speech rate increases as a first encounter progresses. In accordance with the methodological suggestions of that paper, the first 3 min. of each interview were excluded from consideration in the present study.

Another possible explanation is in terms of sentence length. If subjects speak in shorter sentences when they are anxious than when they are not anxious, there would be (proportionate to the number of words in the sample) a greater number of sentences, and therefore a greater number of beginning and end positions. This might account for any increase in the frequency of occurrence for those URSs that occur predominantly at the beginning or the end of the sentence. In order to investigate this possibility sentence length was examined. There was no significant difference in sentence length as a function of anxiety. (Sentence lengths: non-anxious period – mean 11·13, S.D. 2·70; anxious period – mean 11·66, S.D. 2·44; $t = 1{\cdot}08$.)

An alternative explanation, related to the grammatical location of 'Well' and 'I mean', is presented below. When a person is speaking about a topic he finds anxiety-provoking, he may need more time to formulate his thoughts or to choose the appropriate words in which to put them. It has been suggested that verbal planning takes place at a level higher than that of the word and several authors (e.g. Boomer, 1965) suggest that the phonemic clause may be a unit of speech encoding. Lieberman (1965) has discussed the difficulties in the use of phonetic criteria, and sentences were used in this study as the grammatical unit. The location of sentences was in effect determined by an experienced secretary to whom this was a normal task. However, the possibility that she was simply using URSs as criteria for the location of sentences cannot be ruled out. It seems very likely though that anxious speakers need more time at the beginning of sentences to organize their speech. Why they should fill this time, and not simply remain silent, is a question which we have attempted unsuccessfully to answer in previous studies (Lalljee & Cook, 1969; Cook & Lalljee, 1970).

There is no clear relationship between URSs and other forms of disfluency such as filled pauses and non-ah speech disturbances. However, speech rate is highly correlated with 'in fact', 'really', 'sort of' and 'you know'. This suggests that people who speak quickly may do so simply by interpolating more superfluous words into their speech. With the exception of 'well' and 'I mean', URSs that are similarly grammatically located also correlate with one another. This suggests that 'really' and 'sort of' and 'in fact' and 'you know' may fulfil similar functions.

Individual differences in the use of the various hesitation phenomena need exploring. Though sex differences in such a small sample must be treated with caution, the results do show that women use 'I mean' more often than men. This is of special interest in the light of the fact that men use filled pauses more frequently than women. Both filled pauses and 'I mean' may serve the same function cognitively, but their significance for interpersonal communication may be different. For example, the use of 'I mean' rather than 'er' may have overtones of apology, or may be interpreted as the speaker not being sure of himself. Bernstein (1962) suggested that units like 'you know', 'isn't it', etc., serve to reinforce the relationship. It is worth noting that there is a tendency for women to use 'you know' more frequently than men. The investigation of sex differences within this adult subculture and the way in which URSs are actually interpreted by the listener seem promising lines for future development.

REFERENCES

Baker, S. J. (1948). Speech disturbances: a case for a wider view of paraphasias. *Psychiatry* **11**, 359–366.

Bernstein, B. (1962). Social class, linguistic codes and grammatical elements. *Lang. Speech* **5**, 221–240.

Boomer, D. S. (1965). Hesitation and grammatical encoding. *Lang. Speech* **8**, 148–158.

Cook, M. (1969). Anxiety, speech disturbances and speech rate. *Br. J. soc. clin. Psychol.* **8**, 13–21.

Cook, M. & Lalljee, M. (1970). The interpretation of pauses by the listener. *Br. J. soc. clin. Psychol.* **9**, 375–376.

Dibner, A. S. (1956). Cue-counting: a measure of anxiety in interviews. *J. consult. Psychol.* **20**, 475–478.

Feldman, S. S. (1948). Mannerisms of speech. *Psychoanal. Q.* **17**, 356–367.

Feldman, S. S. (1959). *Mannerisms of Speech and Gestures in Everyday Life*. New York: International Universities Press.

Feldstein, S., Brenner, M. S. & Jaffe, J. (1963). The effect of subject sex, verbal interaction and topical focus on speech disruption. *Lang. Speech* **6**, 229–239.

Freud, S. (1914). *Psychopathology of Everyday Life*. London: Benn.

Kasl, S. V. & Mahl, G. F. (1965). The relationship of disturbances and hestiations in spontaneous speech to anxiety. *J. Person. soc. Psychol.* **1**, 425–433.

Krause, M. S. & Pilisuk, M. (1961). Anxiety in verbal behaviour: a validation study. *J. consult. Psychol.* **25**, 414–419.

LALLJEE, M. & COOK, M. (1969). An experimental investigation of the function of filled pauses in speech. *Lang. Speech* **12**, 24–28.

LALLJEE, M. & COOK, M. (1973). Uncertainty in first encounters. *J. Person. soc. Psychol.* **26**, 137–141.

LIEBERMAN, P. (1965). On the acoustic basis of the perception of intonation by linguists. *Word* **21**, 40–54.

MAHL, G. F. (1956). Disturbances and silences in the patient's speech in psychotherapy. *J. abnorm. soc. Psychol.* **53**, 1–15.

SIEGEL, S. (1956). *Nonparametric Statistics for the Behavioural Sciences*. New York: McGraw-Hill.

SIEGMAN, A. W. & POPE, B. (1965). Effects of question specificity and anxiety-producing messages on verbal fluency in the initial interview. *J. Person. soc. Psychol.* **2**, 522–530.

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A NOTE ON INAPPROPRIATE CONSTANCY-SCALING AND THE MÜLLER–LYER ILLUSION

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In a recent issue of *The British Journal of Psychology* Fisher has criticized and Gregory has defended the latter's theory of the distortion illusions (Fisher, 1973; Gregory, 1973). Gregory holds that an object's apparent size is determined not, as has traditionally been supposed, simply by its assumed distance, but also by certain depth cues which operate neural size-scaling mechanisms directly, i.e. without first activating a neural process giving rise to a judgement of the object's distance. One of these cues is 'perspective convergence, of lines or other features such as converging rows of dots. If these depth features are presented on surfaces having no or very different depth, then we should expect *related size distortions to be generated* by inappropriately set scaling. We should expect features such as perspective convergence to set this 'depth-cue' scaling system to *expand* those features which would normally be more distant – for this would be under *normal* conditions of objects in three-dimensional space to give, or help to give, size–distance invariance. But in figures or pictures on flat planes, this scaling – if still set by the perspective features of the figure – *must* produce distortion, because there is no shrinking of the retinal image with perspective *depicted* on a flat plane' (Gregory, 1973, pp. 624–5).

Gregory's theory provides him with an explanation of the Müller–Lyer illusion (Gregory, 1966*a*, *b*). The Müller–Lyer figure, when presented in the vertical plane with the fins open, resembles the perspective view of an inside corner. When presented with the fins closed it resembles the perspective view of an outside corner. We may therefore suppose that the depth–cue size scaling system, operating on the perspective features of the figure, will in the former case expand the shaft of the Müller–Lyer arrow and in the latter case contract it; for the open fins create what might be called a 'receding' perspective, one in which the shaft is more 'distant', and the closed fins an 'approaching' one.

This explanation of the Müller–Lyer illusion is far from unambiguous. In particular, it is not absolutely clear whether the depth-cue size-scaling mechanism scales the relative sizes of objects only within a single perspective, or can in addition or instead relate different perspectives to each other. Now, on the face of it the depth-cue size-scaling mechanism *must* be able to relate different perspectives to each other, and to scale objects in one perspective against objects in another. For the Müller–Lyer illusion can readily be observed when the shaft of an open Müller–Lyer figure is compared with the shaft of a closed one, i.e. when an object within the perspective constituted by one set of fins is compared to an object within the perspective constituted by another set. But this fact presents formidable difficulties to the inappropriate constancy scaling theory. For of two open and upright Müller–Lyer figures with shafts of equal length, that one which has the flatter arrows will present the perspective of the more distant open corner. Hence (contrary to well-established findings) the Müller–Lyer illusion should become greater as the open fins flatten towards 90°.

If, on the other hand, we disregard the above point and assume that the depth-cue size-scaling mechanism scales only the relative sizes of objects within a single perspective, equally formidable problems arise. Let us again take that form of the Müller–Lyer illusion which has or approximates the perspective of an inside corner. Here the 'expansion' of the shaft of the arrow may be regarded as relative to an imaginary line of equal perspective length to the shaft and running parallel to it at the level of the tips of the fins. The depth-cues constituted by the fins will cause the size-scaling mechanism to scale the shaft (the 'corner line') as relatively longer (because relatively more distant) than the imaginary line of equal perspective length near the edge of the perspective field.

The difficulties here are as follows:

(*a*) The inappropriate constancy-scaling theory is once again unable to explain why the Müller–Lyer illusion increases (up to a point) with increasing fin angle. For in the single perspective case, fin angle, once past 90° to the shaft, *ceases to be a perspective cue* to the relative 'separation in depth' of corner line and comparison line. Hence it could not be utilized by a depth-cue size-scaling mechanism to scale corner line and comparison line against each other. That fin angle is not, under these conditions, a cue to relative depth may be seen from the following considerations. Let us assume (with Gregory) that it is only as a *rectangular* corner that the upright Müller–Lyer figure is read, and let us further assume (again with Gregory) that the observer's line of sight bisects the corner angle. Now consider a series of upright and open Müller–Lyer figures drawn in the same (frontal) plane, but with different fin angles. Each one of them can be taken as the perspective of an inside corner of indeterminate height and distance (we are not trying to scale them against each other). If the figures are so taken, then it can readily be seen that the same angular separation of corner line and comparison line in two different figures implies the same *relative* separations-in-depth of observer, corner line and comparison line. It does so *regardless* of any differences in fin angle. For the hypothetical triangle formed by the observer, the corner line and the comparison line has one angle (that of the bisected rectangular corner) which is fixed. If another angle (corner line–observer–comparison line) is now also taken as fixed, the relative distances from the observer to the corner line and from the observer to the comparison line *are thereby determined*. Fin angle has no direct bearing on the matter. It is of course true that since the position of the imaginary comparison line is presumably determined by the position of the tips of the fins, the angular separation of corner line and comparison line will be determined by fin angle and also by fin length if that is not invariant. However, since opening the fins will bring the comparison line closer to the corner line, and should diminish rather than heighten the illusion, this point may be neglected.

(*b*) Increasing the length of the fins increases the perspective distance of the shaft and should therefore cause the depth-cue size-scaling mechanism to 'expand' the shaft proportionately. But optimal fin length is in fact about a quarter of shaft length (Heymans, 1895, cited in Woodworth, 1938, p. 646).

It is perhaps worth remarking that some years ago Mr G. O. Douglas and the present writer carried out some experiments with a luminous Müller–Lyer figure of which the fins were moved in and out by machinery. Operated in darkness, the figure showed marked 'stretching' as the fins opened beyond 90°, but no observer into whose mind the idea had not been put by questioning reported seeing any part of the figure approach and recede. This may be contrasted with Gregory's finding that subjects in the dark presented monocularly with luminous Müller–Lyer figures saw them as having the spatial characteristics of room corners.

REFERENCES

FISHER, G. H. (1973). Towards a new explanation for the geometrical illusions. II. Apparent depth or contour proximity? *Br. J. Psychol.* **64**, 607–621.

GREGORY, R. L. (1966*a*). *Eye and Brain: the Psychology of Seeing.* London: Weidenfeld & Nicolson.

GREGORY, R. L. (1966*b*). Visual illusions. In B. M. Foss (ed.), *New Horizons in Psychology.* Harmondsworth: Penguin Books.

GREGORY, R. L. (1973). A discussion of G. H. Fisher's 'Towards a new explanation for the geometrical illusions: apparent depth or contour proximity?' and the inappropriate constancy-scaling theory. *Br. J. Psychol.* **64**, 623–626.

WOODWORTH, R. S. (1938). *Experimental Psychology.* New York: Holt.

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THE EFFECTS OF NEGATIVE RELATEDNESS UPON WORD–PICTURE AND WORD–WORD COMPARISONS AND SUBSEQUENT RECALL

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In two experiments subjects were required to compare the meanings of either a word and a picture, or of two words. Different name levels, i.e. category versus superordinate names, had only a small effect on the time to compare a name with a picture. When incongruent stimulus pairs were semantically related, both positive and negative decision times were longer than when the incongruent pairs were unrelated; relatedness also affected subjects' recall of stimuli. Implications for models of semantic decision are discussed.

In a recent paper Seymour (1973*a*) has proposed an extension of the logogen model (Morton, 1968) to encompass the data of picture naming and word–picture comparison tasks in addition to word perception. To do so Seymour assumes that picture identification and its observable correlates involve a continuing rather than a unitary process (Bruner, 1957; Wingfield, 1967). In his model the output of perceptual analysers is entered into semantic memory and results in the release of semantic features which are made available to the logogen system. Importantly, the model maintains that the semantic features released in the initial stage of identification are those defining logogens of the generic or perceptual category name of that object. Thus a drawing of a dog is initially identified in terms of features which increment the logogen 'dog' rather than the logogens 'spaniel' or 'animal'. In the model 'dog' would be termed the 'category' name of the picture. Labels of greater specificity or generality (i.e. spaniel or animal) are called 'specific 'and 'superordinate' names respectively, and their logogens are defined by further sets of features whose release is subsequent to the release of categorical features. Release of those features corresponding to a specific name requires additional visual analysis of the stimulus and will not be considered further in this paper. The release of superordinate features, however, is said to result from recoding of the categorical features themselves. For example, if a table is to be classified as an item of 'furniture', the model maintains that the categorical features released in the initial stage must be translated into functional features which define superordinate membership.

By proposing an additional stage between identification at categorical and superordinate levels the model accounts for two empirical findings. First, excepting objects having very low frequency names which will be discussed later, category naming latencies are faster than superordinate naming latencies (Hutcheon, 1970; Segui & Fraisse, 1968; Wingfield, 1967). Secondly, the time taken to compare a picture with a category name is less than that required for comparison of the picture with a superordinate name (Hutcheon, 1970). Since, in the model, both superordinate naming and comparison tasks necessitate the same additional stage of recoding categorical features, it is expected that the increment of superordinate over categorical latencies should be the same in both cases. But for Hutcheon's data this was not true. For in the naming task subjects produced category names 230 msec. faster than superordinate names, but in the comparison task reaction times (RTs) or 'yes' responses to congruent category/picture pairs (CY, e.g. Table/TABLE) were only 40 msec. faster than 'yes' responses to congruent superordinate/picture pairs (SY, e.g. furniture/TABLE). Also, the same relatively small difference in RT was found between 'No' responses to unrelated incongruent category/picture pairs (CN, e.g. Dog/TABLE) and 'no' responses to unrelated incongruent superordinate/picture pairs (SN, e.g. Animal/TABLE). In this respect the model is not supported by Hutcheon's data, but in commenting on the small difference between category and superordinate comparison latencies Seymour (1973*a*) suggested that it might be due to repeated use of a small ensemble of pictures allowing subjects to make effective use of partial visual cues. A primary aim of the first experiment reported in this paper was to retest the model's prediction by collecting data on naming and comparison latencies for a set of 30 pictures, each of which was presented only once for comparison.

When the congruence of two stimuli is judged with respect to a physical dimension, it has been found that increasing the similarity of incongruent pairs results in longer RTs for both positive (congruent) and negative (incongruent) trials (Bindra *et al.* 1968). That is, a decrease in the discriminability of incongruent stimuli makes subjects more cautious about reaching positive decisions. If the same effect occurs when the similarity between two stimuli is semantic rather than physical, this would have serious implications for models which seek to explain semantic comparison latencies in terms of memory structure (Anderson & Bower, 1973; Collins & Quillian, 1969, 1972; Meyer, 1970, 1973) or fixed stages of processing (Clark & Chase, 1972). Such models require that differences in RTs for semantic comparisons represent relatively permanent differences in the number and durations of sequential operations carried out in the memory system. They therefore do not make provision for the possibility that the same comparison may take a longer or shorter time, depending upon the overall difficulty of the task in which it occurs. To investigate this possibility two groups of subjects were run in Expt. I. Both groups received the same positive trials, but for one group incongruent items bore no obvious semantic relationship to one another, while for the other group items were chosen from classes having a close semantic relationship. The expectation was that RTs for both congruent and incongruent comparisons would be increased by the semantic similarity of incongruent pairs.

A supplementary point of interest in the experiment was the subjects' ability to recall the names of pictures presented under the various experimental conditions. Craik & Lockhart (1972) have proposed that the probability of a previously displayed item being recalled is a function of the depth of perceptual-cognitive processing performed on that item at the time of input. If, as Seymour suggests, pictures matched with superordinate names require deeper processing, or additional recoding of features, than those matched with category names, then according to Craik & Lockhart's view those items should be more easily retrieved during subsequent recall. To test this hypothesis subjects were given an unexpected free recall after seeing each picture once during comparison trials, and before going on to give naming responses.

EXPERIMENT I

Method

Subjects

Sixteen male and 16 female subjects were equally assigned to each of the two experimental groups. Subjects were run for one session, lasting approximately 40 min., and were paid for their services.

Stimulus materials

The choice of experimental materials with respect to similarity presented difficulties which must be examined in some detail. The picture stimuli were drawn from six superordinate classes: Animals and Fruit, Furniture and Clothing, Musical Instruments and Tools. While the last four classes are all what Seymour terms 'functional' classes, membership being determined on a basis of similarity of function and not of form, the first two classes might be considered as perceptual classes whose members share certain common physical features. For instance, most animals have eyes, many have legs and fur and so on. In order to avoid confounding perceptual with conceptual interference the choice of items for Animals and Fruit classes was made with careful attention to the visual distinctiveness of the referents. These were dog, cat, penguin, crocodile, kangaroo, and apple, pear, grapes, pineapple, lemon. For the other classes the items depicted were table, chair, sofa, lamp, bed; shoe, coat, dress, trousers, sock; drum, violin, piano, trumpet, harp; axe, hammer, saw, fork, spade.

The second problem to arise in this context concerns the equating of semantic similarity for incongruent category/picture pairs, requiring a 'no' response, (CN) and incongruent superordinate/picture pairs (SN). For CN pairs the situation is straightforward; subjects in the condition in which incongruent pairs contained closely related items, group R, were shown a word and a picture whose referents were of the same superordinate class, e.g. Chair/TABLE, while for subjects in the condition in which incongruent pairs contained unrelated items, group U, the referents were always members of different classes, e.g. Chair/APPLE. For SN pairs, however, there is no comparable solution: while the superordinates Animals and Fruit can be distinguished by both being nested under 'Living Things', groupings of the other superordinates into related clusters is less obvious. However, Musical Instruments and Tools have much in common, being manipulanda, thus forming a second superordinate cluster and leaving Furniture and Clothing as the third cluster. A related SN pair (SN$_r$) was defined as one in which the name of one class in a superordinate cluster was paired with a picture from the other class in that cluster, e.g. Fruit/DOG, or Clothing/CHAIR. Then for unrelated SN trials (SN$_u$) the name of a superordinate class was paired with a picture from a class not in the same cluster, e.g. Fruit/CHAIR or Clothing/DOG. Similarly, for unrelated CN pairs (CN$_u$) the picture and the word were always from different classes which did not form a superordinate cluster, e.g. Dog/CHAIR but not Dress/CHAIR. Thus for group R who received related items in incongruent pairs the degree of relatedness was necessarily greater for category comparison trials than for superordinate comparison trials.

Stimulus pictures were small black and white pictures cut from children's reading books and dictionaries, they were stuck on to strips of white card which could be fitted into specially made frames, so that the picture was visible through an aperture in the front of the frame. The 30 experimental pictures were as listed above. For practice there were a further 16 pictures, two of which did not belong to any of the six superordinate classes. The words with which the pictures were to be matched were either category or superordinate names. At the superordinate level only the names of the six relevant classes were used, both for congruent and incongruent pairs. Category names for CY pairs were, of course, appropriate to the picture shown on that trial. For CN pairs the words were always the names of objects which were members of the six classes but for which there was no corresponding picture. The frequency of the words used ranged from AA to 2/million as estimated by the Thorndike–Lorge count (1944).

Picture stimuli were viewed by the subject in an English Electronic three-field tachistoscope and subtended an angle of approximately 3°. The subject wore a throat microphone which was attached to a voice-key and a Venner millisecond timer. Presentation of the word stimuli involved two departures from the methodology of Hutcheon's study. Whereas she presented words visually and simultaneously with a picture, in Expt. I presentation of the word was auditory and preceded onset of the picture by one second. Specifically, the name to be matched was given in a question asked by the experimenter. Thus, for example, 'Is this a dog'? or 'Is this an animal?'

Presentation orders for the stimuli were as follows. Four lists of the 30 pictures were constructed such that each picture occurred once in each of the four sub-conditions, CY, SY, CN, SN, and so that each of the six classes was about equally represented in every sub-condition on each list. Names were then paired with the picture stimuli, one set for the U condition and another for R. Since 30 is not divisible by 4 there were two lists containing 16 superordinate comparisons and 14 category comparisons with the opposite arrangement for the remaining two lists. For each list there were four random orders of trials, so that each subject within a condition received sub-conditions in a unique order but was matched for orders of both sub-conditions and pictures with a subject in the other group. Thus matched subjects, always of the same sex, differed on the type of incongruent pairs, related or unrelated, but received identical congruent pairs at the same positions within a presentation order. During practice all subjects had exactly the same order of sub-conditions and pictures but with the words of incongruent pairs varying as appropriate to each condition.

Procedure

Prior to the experimental trials the voice key was adjusted for each subject, who then gave six simple reaction times to a blank card which replaced the fixation card, responding 'yes' three times and 'no' three times. Half the subjects gave 'yes' responses first while the other half said 'no' first. Instructions as to the main task were then given; subjects in the U group were told that items on negative trials would not be confusable, while the R group received no information on this point. Each subject was then run for 30 trials without replacement of trials on which errors occurred. On each trial the subject was instructed to ready himself and then the experimenter asked the appropriate question. One second later the fixation field was replaced by a picture for 0·5 sec. and then by a blank card. Two minutes after the last trial the subject was given pencil and paper and asked to write down the names of as many of the objects depicted in the test as he could remember. Three minutes was allowed for recall. When the recalled items had been removed the subject was read instructions for the naming task. Half of each group, R and U, was assigned to the 'category' or 'free' naming condition and half to 'superordinate' naming. For the former condition the subject was told to respond to each stimulus with the first name for it which occurred to him. Subjects in the 'superordinate' group were read and made to repeat the names of the six classes; they were told that every stimulus belonged to one of the classes and that they were to respond with the appropriate class name. Practice for both groups was given with the same pictures used for practice on the comparison task, less the two which did not belong to any of the six classes. Each subject in a group named the pictures in a different random order, but in the same order as one subject in the other group. A 1 sec. warning was given prior to the onset of a stimulus, which then appeared for a further 1 sec.

Results

The simple RTs obtained from each subject prior to the comparison task were averaged separately for 'yes' and 'no' responses. 'Yes' responses were longer than 'no' responses by 6 msec. overall, and this difference was not significant ($t = 0{\cdot}65$; d.f. = 31). Thus differential sensitivity of the apparatus to the two responses was not a factor in subsequent RT measurement.

Naming latencies

The mean latencies to name the pictorial stimuli were 755 msec. (S.D. 65 msec.) for category naming, and 947 msec. (S.D. 99 msec.) for superordinate naming. As expected, the magnitude of the difference between the two name levels was inversely related to the Thorndike–Lorge frequency of the names of the depicted objects (Oldfield & Wingfield, 1965). The overall difference (192 msec.), however, is close to that reported by Hutcheon and provides a reasonable figure against which to compare the difference between RTs for category/picture and superordinate/picture comparisons.

Matching latencies

Wingfield (1968) has found that when a category name and a picture, or two pictures, are presented successively for comparison there is no effect of the Thorndike–Lorge frequency of either stimulus on RT. On the other hand, Egeth & Blecker (1971) showed that comparison RTs for simultaneously displayed letter pairs was dependent on letter frequency, while Seymour (1973*b*) reported a similar effect for simultaneous presentation of a geometrical shape and its name, and for two shapes. Since in Expt. I words and pictures were presented successively, the data should agree with Wingfield's finding that stimulus frequency and RT are independent. Each subject's data for CY and SY trials were accordingly partitioned with respect to picture frequency, so that mean RT to the three or four highest frequency pictures shown to him within a sub-condition could be contrasted with his mean RT to the three or four lowest frequency pictures presented in that sub-condition. For both sub-conditions CY and SY there was some slight advantage to the higher frequency items, but for neither category comparisons ($t = 1{\cdot}25$; d.f. $= 31$) nor superordinate comparisons ($t = 0{\cdot}09$; d.f. $= 31$) did the difference approach significance. The comparison latency data of the experiment can therefore be interpreted without regard to possible frequency effects. Similarly, the low error rates of both groups of subjects (2·3 per cent for U and 2·7 per cent for R) indicate that none of the recorded differences between condition means are likely to have resulted from speed/error trade-offs.

Table 1 shows the mean RTs of both groups for each sub-condition, the figures being derived from each subject's mean RTs. Hutcheon's finding that CY comparisons are faster than SY comparisons has been replicated in both groups of subjects, but for neither group U nor group R does this effect approach the difference (192 msec.) between the categorical and superordinate naming latencies. It can be seen that similar differences exist between CN and SN. Thus although the name level effect is extremely significant by analysis of variance ($F = 43{\cdot}17$; d.f. $= 1, 30$; $P < 0{\cdot}001$), its absolute value is very much lower than that predicted from Seymour's model.

Table 1 also shows that whereas decisions of incongruency are about as fast as decisions of congruency for group U, they are considerably slower for group R. This is confirmed in the analysis by a significant main effect of response, 'yes' or 'no' ($F = 7{\cdot}83$; d.f. $= 1, 30$; $P < 0{\cdot}01$), and by the only significant interaction of the analysis, response × incongruent-pair relatedness ($F = 4{\cdot}63$; d.f. $= 1, 30$; $P < 0{\cdot}05$). It is clear, however, from the significant main effect of incongruent-pair relatedness ($F = 9{\cdot}19$; d.f. $= 1, 30$; $P < 0{\cdot}01$) that the introduction of relatedness between members of incongruent pairs increases RTs for congruent as well as incongruent pairs. This is confirmed by the results of a separate analysis of variance performed on the SY and CY data only which indicated significant effects of name level ($F = 18{\cdot}42$; d.f. $= 1, 30$; $P < 0{\cdot}001$) and incongruent-pair relatedness ($F = 4{\cdot}68$; d.f. $= 1, 30$; $P < 0{\cdot}05$), and in addition a significant interaction between these two factors ($F = 4{\cdot}35$; d.f. $= 1, 30$; $P < 0{\cdot}05$) which is due to a greater effect of relatedness on RTs of SY pairs than on RTs of CY pairs. The results of this second analysis confirm

Table 1. *Mean RTs (in msec.) for positive and negative comparisons of a picture and a category or superordinate name, for related and unrelated negative groups*

	Yes		No	
	Category CY	Superordinate SY	Category CN	Superordinate SN
Unrelated negatives U	525	547	523	557
Related negatives R	555	618	601	636

Table 2. *Recall scores, as percentages, by trial types for two groups of subjects, and absolute group totals*

	Yes		No		Total no. items recalled
	Category	Superordinate	Category	Superordinate	
Unrelated U	59	65	28	28	217
Related R	50	55	44	36	222

that congruent RTs are sensitive to manipulation of incongruent pair difficulty, and the interaction between relatedness and name level suggests the effect is not due solely to a general increase in reluctance to respond. The implications of these findings for theories of semantic decisions will be discussed later.

Recall data

The percentage recall score of each subject for each sub-condition was calculated, but with items which had involved error responses during the comparison task excluded. Group mean percentages are shown in Table 2, which also includes the total number of items recalled by each group. A three-way analysis of variance was performed on the percentage scores with one between-subjects factor, group (U or R) and within-subjects factors of name level (category or superordinate) and response (yes or no). Of the main effects only that of response was significant ($F = 50{\cdot}87$; d.f. $= 1, 30$; $P < 0{\cdot}001$), reflecting the superior recall of the names of pictures which were seen in congruent pairs. Although the congruence/incongruence of a comparison was the most important determinant of whether or not an item would be recalled, the analysis of variance also revealed one significant interaction which was that of groups × response ($F = 10{\cdot}3$; d.f. $= 1, 30$; $P < 0{\cdot}01$).

This interaction reflects the fact that while group U recalled more than twice as many congruently as incongruently matched items, for group R the advantage to congruence was considerably reduced. Group R recalled many more incongruently matched items than did group U but fewer congruently matched items, so that group recall totals were almost equal.

The absence of a main effect for name level suggests that, counter to Seymour's assumption, pictures were not more completely encoded for superordinate than for category comparisons, or according to the depth of processing notion they would have been better recalled. The greater recall of SY than CY items by both groups of subjects indicates that there might have been a name level effect for congruently matched pictures only, but the difference is not significant ($t = 1{\cdot}16$; d.f. $= 31$).

EXPERIMENT II

The comparison RT data of Expt. I illustrate that the time taken to indicate the congruence of a pictured object and one of its names is determined not only by the relationship between the two stimuli, but also by the difficulty of those trials on which the stimuli are incongruent. Because this finding implies serious restrictions on the interpretation of reaction time structures derived from semantic comparison tasks, it was decided to test the replicability of the effect within a somewhat different paradigm. Subjects in group U had been informed that they would not encounter semantically confusable stimulus pairs and this may have made them more ready to respond, thus lowering their response times relative to group R; in Expt. II, therefore, no prior information was given on this point. Secondly, relatedness of items in incongruent pairs was now run as a within-subjects variable of three levels. These levels were represented by conditions of related (R), mixed (M) and unrelated (U) stimuli comprising incongruent pairs; in the mixed condition half the incongruent pairs, requiring 'no' responses, contained related items (N_{mr}) and half contained unrelated items (N_{mu}). Finally Expt. II differed from Expt. I in that only word stimuli were employed, thus making Expt. II comparable with previous studies of semantic decisions (e.g. Schaeffer & Wallace, 1970; Wilkins, 1971), and they were presented simultaneously rather than successively. With so many differences from Expt. I it was felt that Expt. II should adequately test the robustness of the present effect.

Method

Subjects

Six female and six male subjects each participated in three sessions, corresponding to the three conditions of related, mixed and unrelated negative trials. A session lasted approximately 20 min., and subjects were paid for their services.

Materials

Four classes of words were selected from the Battig & Montague (1969) association norms so as to form two related pairs. The classes were Fruit and Flower, Bird and Four-legged Animals; the latter was named 'Mammal' for the purposes of the experiment. The choice of items all from the same semantic domain of 'living things' was deliberate, as it seemed that extreme variations in the constitution of negative trials between sessions might cause subjects to guess the experimental hypothesis and purposefully fulfil it. From each class 14 responses were selected, the first 10 given in the norms were assigned to be experimental items and the next four to be practice items. Twelve random orders of the experimental words were formed such that each word was to be congruously paired with its class name on half the lists and incongruously with another class on the other half. Three copies of each list were prepared, one for each condition, and class names were added to the list as appropriate for congruent and incongruent pairs; these occurred in random order, as did the related and unrelated incongruent pairs in the lists for condition M. Two subjects were assigned to each of the six possible orderings of the three conditions, and each subject received a different basic list of exemplars for each condition. The exemplar and class words were presented simultaneously in different channels of the tachistoscope, the class word appearing over the fixation point and half an inch above the exemplar at a viewing distance of 20 in.

Procedure

Subjects were run for three sessions on different days. They were instructed to respond 'yes' or 'no' depending on whether the words were congruent or not, and always to fixate the

Table 3. *Mean RTs and percentage errors for positive (Y) and negative (N) matches, for related (R), mixed (M) and unrelated (U) conditions. (RTs in msec.)*

	Y			N			
	r	*m*	*u*	*r*	*mr*	*mu*	*u*
Mean RT	887	829	814	1001	957	867	872
Errors (%)	3·3	4·2	4·2	11·3	15·0	4·2	2·9

upper, class word first. A trial consisted of a 1 sec. warning, followed by presentation of the words for 1·5 sec. Vocal responses operated a voice key and timer. A session began with 16 practice trials, negatives being appropriate to the particular conditions, and then followed 40 experimental trials.

Results

Error rates and mean correct RTs for each condition are shown in Table 3. Scores are based on the mean RTs of each subject, and it may be noted that the means for related and unrelated incongruent pairs in condition M (N_{mu} and N_{mr}) represent only half as many observations, as do all other means. It is clear from Table 3 that once again 'yes' RTs to congruent pairs are sensitive to changes in the difficulty of incongruent pairs. An analysis of variance was performed on the 'yes' data with a between-subjects factor (order of session) and a within-subjects factor (conditions). Only the main effect of conditions was at all significant ($F = 4·38$; d.f. = 2, 12; $P < 0·05$). A Newman–Keuls test indicated that Y_u and Y_m were both significantly different from Y_r at $P < 0·05$, but not from each other. A second analysis of variance was performed on the 'no' data and again only the conditions effect was significant ($F = 8·76$; d.f. = 3, 18; $P < 0·001$). A Newman–Keuls test showed that both N_u and N_{mu} were significantly different from N_r ($P < 0·01$) and N_{mr} ($P < 0·05$). Despite the 44 msec. difference between N_r and N_{mr} neither this comparison nor that between N_u and N_{mu} was significant. Thus this data shows only that related negatives are associated with longer RTs than are unrelated negatives (Collins & Quillian, 1970; Wilkins, 1971).

DISCUSSION

Comparison latencies

The results of Expt. II confirm the finding in Expt. I of an effect of the difficulty of incongruent trials upon the absolute RTs for congruent comparisons. In both cases RTs for 'yes' responses, indicating congruence, were increased by the presence of semantic similarity between items constituting incongruent pairs. It is interesting to note that 11 of the 12 subjects in Expt. II were unable, when asked, to identify any change in the experimental conditions across sessions: all assumed the experimenter was interested in practice effects. The changes in RT do not therefore require a subject's conscious awareness of variations in overall task difficulty.

The finding of an influence upon 'yes' RTs of what may be termed the task difficulty effect is important because it complicates the interpretation of RT structures. It has particularly strong implications for theories which attempt to explain semantic comparison times in terms of the organization and retrieval of information stored in semantic memory (Anderson & Bower, 1973; Collins & Quillian, 1969, 1972; Meyer, 1970, 1973) and of fixed processing stages (Clark & Chase, 1972). Since none of these theories, as presently formulated, allow that a particular congruence decision may have a variable duration, they cannot account for the fact that the relationships between RTs for various conditions may, at least in part, result from the manner in which those conditions are combined in an experimental session.

An alternative approach is to place substantial emphasis on the decision-making aspects of the tasks (Rips *et al.*, 1973; Schaeffer & Wallace, 1970). One direction such an approach can take is to follow Seymour (1973*a*) by accepting that graphemic and pictorial features of word and picture stimuli are dealt with by sets of nodes (logogens) which provide access to semantic memory; semantic features associated with the stimuli are then made available for further processing. The two sets of semantic features derived from the stimulus pairs are entered into a comparator where they are examined for communality. The outcome of each feature comparison is communicated to a decision-maker in which a cumulative record of matches and mismatches is kept. The decisions 'congruent' or 'incongruent' are made for a stimulus pair when the amount of evidence exceeds the appropriate criterion level, which can be set independently for match and mismatch counts depending on the subject's readiness to commit errors. The model accounts for longer RTs to related as compared with unrelated incongruent pairs because with the former a proportion of the feature comparisons have positive outcomes, so that the mismatch count reaches criterion more slowly than for unrelated pairs. Similarly the model explains the name level effect on congruent decisions in terms of differential count rates. Since the semantic features made available to the comparator by category/PICTURE pairs, e.g. apple/APPLE are more similar than those made available by superordinate/PICTURE pairs, e.g. fruit/APPLE, the count of matching features should increase faster for category than for superordinate comparisons. The same explanation also applies to the dominance effect reported by Wilkins (1971).

The task difficulty effect is accounted for in the model by adjustments in the positive count criterion. When incongruent stimulus pairs contain unrelated items subjects are able to accept quite low match counts as evidence of congruence, since even low counts are unlikely to arise from incongruent pairs. In conditions of related incongruent pairs, however, low match counts may arise from such pairs, so that only a raised match count criterion will differentiate congruent from incongruent stimulus pairs. Therefore to maintain equal levels of performance under related and unrelated conditions, the latencies of congruent decisions for the former case must be increased due to a higher setting of the match counter. It should be noted here that equivalent changes in the mismatch criterion are not logically necessary. Reducing the discriminability of incongruent stimuli raises only the evidence requirements for positive but not negative decisions; a given degree of dissimilarity will be sufficient for the latter decision whatever the similarity of the stimuli. Equivalent effects on incongruent RTs should presumably be possible if the difficulty of congruent comparisons were systematically varied. Additional evidence in favour of the model is the interaction between name level and task difficulty for 'yes' RTs in Expt. I. The effect of task difficulty was greater for superordinate comparisons than for the faster category comparisons. This is explained by the differential effects which raising the match criterion has on the two types of trial. For since evidence of congruence is recruited less rapidly for superordinate than for category comparisons, as described above, the effect of increasing the match count criterion will be greater on the former than on the latter.

The model just described has much in common with that put forward by Schaeffer & Wallace (1970), although they propose that decision criteria change on each trial following an initial holistic comparison of the features associated with the stimuli. The present results, while substantially in agreement with Schaeffer & Wallace's ideas, argue that criterial levels may be fixed at a particular value established during practice and that differences in RTs – for instance, between related and unrelated negatives – result from differential rates of growth in the balance of evidence for congruency and incongruency being recruited from the comparator. But it is quite possible that factors such as task difficulty serve to set up an initial criterial level which thereafter varies from trial to trial in the manner discussed by Schaeffer & Wallace. We have no means of distinguishing between these alternatives given the present data, but the two experiments strongly support an analysis of semantic decision times which emphasizes comparison processes rather than features of storage and retrieval systems.

The original impetus of Expt. I derived from Seymour's suggestion that, given a large ensemble of pictures rather than repeated presentations of the same small ensemble, the difference between comparisons at categorical and superordinate name levels would equal the difference in the respective naming latencies. This was found not to be the case. Seymour's prediction was based on the assumption that both superordinate naming and comparison require an extra coding stage compared to tasks involving category labels, but the assumption that differences in naming latencies offer a basis for predicting differences in comparison latencies rests on unsure grounds. That naming latencies to pictures do not by themselves tell us much about the duration of stimulus encoding and conceptual identification is well known. Latencies of this sort reflect response rather than stimulus factors, and their magnitudes may be determined by the frequency of the response (Oldfield & Wingfield, 1965), age of acquisition of the response (Carroll & White, 1973) or the compatibility of the required response with the stimulus (Wingfield, 1967; Fraisse, 1969). The comparison latencies of Hutcheon's experiment and of Expt. I show that identifying an object at a superordinate level need only be slightly longer than identifying it at a category level, but any of the variables just cited may magnify the difference when conceptual identification has to be translated into a nominal output.

An example of the confounding effects of response factors is given by objects whose category names are of very low frequency. As mentioned in the introduction, in naming pictures of such objects Wingfield (1967) found that the usual category–superordinate difference is reversed, yet he found no effect of frequency on category/PICTURE comparison latencies (Wingfeld, 1967). Response factors which are precluded from the comparison task do therefore become operative in a naming situation, and such factors may be presumed to underlie the discrepancy between the data of Expt. I and the prediction initially derived from Seymour's model. Furthermore, this model has difficulty in explaining the interaction between name level and task difficulty which was observed for the 'yes' RTs of Expt. I.

According to subtractive logic (Sternberg, 1969), when two factors interact they do so because of their influence on a common processing stage, which in Seymour's model would have to be the encoding stage. While it is not inconceivable that increasing the semantic similarity of incongruent pairs should cause subjects to spend longer encoding the stimulus pictures, it is not clear what this would mean in terms of the model. The criterion model described above adequately explains this interaction by maintaining that name level and task difficulty are influential at the decision stage.

Recall data

The recall data shown in Table 2 offer further support for the idea that comparing an object against a superordinate name rather than a category name does not involve an additional coding stage, since the name level does not influence item recall as predicted by the levels of processing notion. Although most of the supportive evidence for levels of processing theory is based on gross differences of physical versus semantic processing (Craik, 1973; Hyde & Jenkins, 1969; Schulman, 1971), evidence has been found of more subtle effects due to differing degrees of semantic coding. Thus Gardiner (1974) found that target items which had been classified as 'precious metals' were subsequently better recalled than those which had merely been classified as 'metals'. The recall data of Expt. I are somewhat discrepant with Gardiner's data, but in his experiment, as in those others cited above, deeper processing was correlated with greater specificity of the retrieval cues provided by the classes, whereas in Expt. I this was not so; all items, whether involved in category or superordinate comparisons, were members of the same set of classes.

The greater memorability of congruently paired stimuli relative to incongruently paired items replicates the findings reported by Schulman (1971) using words and recognition testing, but the strong interaction between response and relatedness represents a novel finding: items from related incongruent pairs are recalled better than items from unrelated pairs, although this happens at the expense of the recall of congruently paired stimuli. It may be that related items are better registered in memory as a result of the longer time for which they are processed but since congruent comparisons have the fastest RTs, the high recall of congruently paired items would have to be explained in terms of a separate principle. A parsimonious explanation derives from the encoding specificity theory (Tulving & Osler, 1968; Thomson & Tulving, 1970). According to this view, a retrieval cue is effective to the extent that items are encoded at input with respect to the cue; in the present context knowledge of the superordinate ensemble provides a plausible basis for cueing at recall. It seems credible that the encoding specificity principle is revealed in the comparison situation by the fact that the rank ordering of recall performance, items from congruent pairs, items from related incongruent pairs, and items from unrelated incongruent pairs, is the same as the ordering of the number of matching features arising in the comparison process. If it is assumed that feature comparison has memorial consequences, then items from congruent comparisons will have more features encoded with respect to the cue, i.e. matching features, than will items from incongruent comparisons – related and unrelated in that order.

This explanation is embarrassed by the finding that recall of items from congruent pairs is greater for group U than for group R, since the raising of the decision criterion on the feature match count for group R should lead to an increase in the number of matches accumulated before a decision is made. It is possible that recall of items from any one superordinate class is limited (Tulving & Pearlstone, 1966) and some trade-off takes place between the recall of items from the various pairings. The equivalence of the total recall scores for groups U and R supports this view. We have further data from another experiment which indicate that with larger numbers of semantic classes recall of both incongruent and, to a lesser degree, congruent items is enhanced by the presence of related incongruent pairs.

REFERENCES

ANDERSON, J. R. & BOWER, G. H. (1973). *Human Associative Memory*. Washington, D.C.: Winston.

BATTIG, W. F. & MONTAGUE, W. E. (1969). Category norms for verbal items in 56 categories: a replication and extension of the Connecticut category norms. *J. exp. Psychol. Monogr.* **80**, 1–46.

BINDRA, D., DONDERI, D. C. & NISHISATO, S. (1968). Decision latencies of 'same'–'different' judgements. *Percept. Psychophys.* **3**, 121–130.

BRUNER, J. S. (1957). On perceptual readiness. *Psychol. Rev.* **64**, 123–152.

CARROLL, J. B. & WHITE, M. N. (1973). Word frequency and age of acquisition as determiners of picture naming latency. *Q. J. exp. Psychol.* **25**, 85–95.

CLARK, H. H. & CHASE, W. G. (1972). On the process of comparing sentences against pictures. *Cogn. Psychol.* **3**, 472–517.

COLLINS, A. M. & QUILLIAN, M. R. (1969). Retrieval time from semantic memory. *J. verb. Learn. verb. Behav.* **8**, 240–247.

COLLINS, A. M. & QUILLIAN, M. R. (1970). Experiments on semantic memory and language comprehension. In. L. W. Gregg (ed.), *Cognition in Learning and Memory*. New York: Wiley.

CRAIK, F. I. M. (1973). A 'levels of analysis' view of memory. In P. Pliner, L. Krames & T. M. Alloway (eds.), *Communication and Affect: Language and Thought*. New York: Academic Press.

CRAIK, F. I. M. & LOCKHART, R. S. (1972). Levels of processing: a framework for memory research. *J. verb. Learn. verb. Behav.* **11**, 671–684.

EGETH, H. & BLECKER, D. (1971). Differential effects of familiarity on judgements of sameness and difference. *Percept. Psychophys.* **9**, 321–326.

FRAISSE, P. (1969). Why is naming longer than reading? *Acta psychol.* **30**, 96–103.

GARDINER, J. M. (1974). Levels of processing in word recognition and subsequent free recall. *J. exp. Psychol.* **102**, 101–106.

HUTCHEON, E. G. (1970). An investigation into stimulus classification under varying instructions. (Unpublished M.A. thesis, University of Dundee.)

HYDE, T. S. & JENKINS, J. J. (1969). Differential effects of incidental tasks on the organization of recall of a list of highly associated words. *J. exp. Psychol.* **82**, 472–491.

MEYER, D. E. (1970). On the representation and retrieval of stored semantic information. *Cogn. Psychol.* **1**, 242–300.

MEYER, D. E. (1973). Correlated operations in searching stored semantic categories. *J. exp. Psychol.* **99**, 124–133.

MORTON, J. (1968). Grammar and computation in language behaviour. (Progress report no. 6, Centre for Research in Language and Language Behaviour, University of Michigan.)

OLDFIELD, R. C. & WINGFIELD, A. (1965). Response latencies in naming objects. *Q. J. exp. Psychol.* **17**, 273–281.

RIPS, J. L., SHOBEN, E. J. & SMITH, E. E. (1973). Semantic distance and the verification of semantic relations. *J. verb. Learn. verb. Behav.* **12**, 1–20.

SCHAEFFER, B. & WALLACE, R. (1970). The comparison of word meanings. *J. exp. Psychol.* **86**, 144–52.

SCHULMAN, A. I. (1971). Recognition memory for targets from a scanned word list. *Br. J. Psychol.* **62**, 335–346.

SEGUI, J. & FRAISSE, P. (1968). Le temps de réaction verbale. III. Réponses spécifiques et réponses catégorielles à des stimulus objets. *Année psychol.* **68**, 69–82.

SEYMOUR, P. H. K. (1973*a*). A model for reading, naming and comparison. *Br. J. Psychol.* **64**, 35–49.

SEYMOUR, P. H. K. (1973*b*). Semantic representation of shape names *Q. J. exp. Psychol.* **25**, 265–277.

STERNBERG, S. (1969). The discovery of processing stages: extensions of Donders' method. *Acta psychol.* **30**, 276–315.

THOMSON, D. M. & TULVING, E. (1970). Associative encoding and retrieval: weak and strong cues. *J. exp. Psychol.* **86**, 255–262.

THORNDIKE, E. L. & LORGE, I. (1944). *The Teacher's Word Book of* 30,000 *Words*. New York: Columbia University Press.

TULVING, E. & OSLER, S. (1968). Effectiveness of retrieval cues in memory for words. *J. exp. Psychol.* **77**, 593–601.

TULVING, E. & PEARLSTONE, Z. (1966). Availability versus accessibility of information in memory for words. *J. verb. Learn. verb. Behav.* **5**, 381–391.

WILKINS, A. (1971). Conjoint frequency, category size, and categorization time. *J. verb. Learn. verb. Behav.* **10**, 382–385.

WINGFIELD, A. (1967). Perceptual and response hierarchies in object identification. *Acta psychol.* **26**, 216–226.

WINGFIELD, A. (1968). Effects of frequency on identification and naming of objects. *Am. J. Psychol.* **81**, 226–234.

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CONTEXT-DEPENDENT MEMORY IN TWO NATURAL ENVIRONMENTS: ON LAND AND UNDERWATER

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In a free recall experiment, divers learnt lists of words in two natural environments: on dry land and underwater, and recalled the words in either the environment of original learning, or in the alternative environment. Lists learnt underwater were best recalled underwater, and vice versa. A subsequent experiment shows that the disruption of moving from one environment to the other was unlikely to be responsible for context-dependent memory.

The philosopher John Locke cites the case of a young man who learned to dance in a room containing an old trunk. Unfortunately, however, 'the idea of this remarkable piece of household stuff had so mixed itself with the turns and steps of all his dances, that though in that chamber he could dance excellently well, yet it was only while that trunk was there; nor could he perform well in any other place unless that or some other trunk had its due place in the room' (Locke, cited in Dennis, 1948, p. 68).

The belief that what is learnt in a given environment is best recalled in that environment has of course been a useful standby for detective story writers from Wilkie Collins onwards, although the empirical evidence for such a belief is somewhat equivocal. Farnsworth (1934) and Pessin (1932) were both unable to obtain a context-dependent memory effect. A later study by Jensen *et al.* (1971) was more successful, but a recent unpublished study by Hitch (personal communication) failed to observe any effect. An alternative approach to the context-dependent phenomenon utilizes a retroactive interference (RI) design in which material learned in one environment is followed by a second set of material presented in either the same or a different environment, which in turn is followed by a recall test on the original material. This final test itself may be in the initial environment or in the interpolated environment. Using this design, Bilodeau & Schlosberg (1951) found that an interpolated list caused only half as much RI when it was learned in a room differing markedly from that in which original learning took place. Comparable results were obtained by Greenspoon & Ranyard (1957), and by Zentall (1970). However, Strand (1970) has presented evidence suggesting that the inferior retention observed when the recall environment is different results not from the different context *per se*, but from the disruption that occurs when the subject moves from one environment to the other. She required the subjects in her control conditions, in which learning and recall were in the same environment, to leave the room and have a drink of water from a drinking fountain before beginning the recall phase of the experiment. Under these conditions she found no reliable difference between subjects who learned the interfering material in the same and those who learned in a different environment.

The evidence for context-dependent memory is therefore far from convincing. Furthermore, a number of the studies which have obtained effects have used extremely artificial environments, suggesting that any effect observed may not generalize beyond the conditions under which the experiment was run. Dallett & Wilcox (1968), in one of their conditions, required their subjects to stand with their heads inside an oddly shaped box containing flashing lights of different colours (two subjects had to be excused due to nausea), while in a further study Rand & Wapner (1967) strapped their subjects to a board which was then rotated so as to keep the subject either supine or erect. As Brunswik (1956) has pointed out, it is important for the psychologist to check the 'ecological validity' of his results by ensuring that they are applicable to the real world, and are not limited to the artificial conditions of the psychological laboratory. The experiments to be described are therefore concerned with investigating the phenomenon of context-dependent memory in two natural environments. Egstrom *et al.* (1972) observed that divers had considerable difficulty in recalling material learnt under water, and since recognition memory was not impaired in this way, they suggested that the defect was probably due to a context-dependent memory effect rather than to differential learning underwater. Unfortunately, since the appropriate controls were not included, such a conclusion was purely speculative. However, the underwater environment does present a particularly good example of a natural environment which differs dramatically from that on the surface. The diver underwater is weightless, has restricted vision, and subjectively is in an environment which is as different from the surface as any he is ever likely to experience. Divers were therefore asked to learn word lists both on land and underwater and subsequently recalled either on land (Dry) or underwater (Wet). Each diver performed under all four possible conditions: DD (Learn Dry, Recall Dry); DW (Learn Dry, Recall Wet); WW and WD. Should the phenomenon of context-dependent memory exist under these conditions, performance when learning and recall took place in the same environment (DD and WW) should be significantly better than when recall took place in a different environment to that of learning (DW and WD).

EXPERIMENT I

Method

Subjects

Eighteen subjects were tested, comprising 13 male and five female members of a university diving club.

Apparatus

Five lists of words, each consisting of 36 unrelated, different, two- and three-syllable words chosen at random from the Toronto word bank, were constructed and subsequently recorded on tape (see *Procedure*).

Two Diver Underwater Communication (DUC) sets were used. A DUC set consists of a surface-to-diver telephone cable, terminating in a bone transducer, which, placed on the diver's mastoid, enables both surface-to-diver and diver-to-surface communication. The two DUC sets were slightly amended such that taped material, monitored by the surface operator, could be presented directly to the subject using a cassette tape-recorder. Twin transducers on each set allowed two separate pairs of subjects to be tested during the same period. Weighted formica boards, sealed with fablon, enabled subjects to record responses in pencil both above and underwater. Subjects used standard SCUBA breathing apparatus and diving equipment of various designs dictated by personal preference.

Procedure

All instructions and stimuli for each experimental session were recorded on tape. Efficient auditory perception of stimuli by a submerged diver using SCUBA apparatus is seriously impaired by the noise of his breathing. The presentation of the material was therefore grouped, so as to allow the diver to adopt a comfortable breathing rate which did not interfere with his auditory perception. Thus, each list was presented in blocks of three words. Within each block, the words were spaced at 2 sec. intervals. Between each block, a 4 sec. interval enabled subjects to exhale, inhale and hold their breath in readiness for the presentation of the next block, and so on.

Each tape began with an explanation of this breathing procedure, followed by a 'breathing pattern' section, to ensure that subjects were breathing correctly and in rhythm before the first word of the list appeared. This section consisted of nine spoken presentations of the letter z, in three blocks of three, and with identical spacings to those of the words in the list itself. Immediately after each block of z's, subjects heard the command 'breathe'. The presentation of the word list followed on naturally in rhythm, and the command to breathe was then dropped.

On each tape (one for each of the 16 condition/list combinations), the relevant list was presented twice. Between the first and second presentations, a gap of 10 sec. allowed subjects a short rest with unconstrained breathing. The second presentation was again preceded by the breathing pattern section.

To eliminate possible primary memory effects, the second presentation of each list was followed by 15 digits which subjects were required to copy at a rate of two seconds per digit. This was followed by the next instruction (e.g. 'Ascend to the shore station'), and a 4 min. delay. This recurred in all conditions and was necessary to enable subjects to comply safely with the relevant instruction. They were then instructed to write down in any order as many of the words from the list as they could remember. Two minutes were allowed for this, after which the session was terminated.

The original 16 subjects were split at random into four groups of four. Prior to the first experimental session, all subjects underwent a practice session, comfortably seated around a table. During this, they first practised the breathing technique, then the task itself, using a practice list.

Pairings of the remaining four lists $L_1 \ldots L_4$ with the four conditions, and the temporal orderings of the conditions for each of the four groups, were arranged according to a Graeco-Latin square design. Subjects experienced one condition per diving session, and the sessions were separated by approximately 24 hr. The design was such that each group experienced conditions and lists in different orders, that a given condition/list pair was never administered to more than one group, and that lists and conditions had equal representation on each experimental session.

Testing took place at open water sites near Oban, Scotland, in most cases as soon as subjects returned from their scheduled dive for the day. The latter consideration helped to ensure that subjects began each session in roughly the same state, that is, wet and cold. They were tested in pairs which remained the same across all sessions.

Subjects in environment D (Dry) sat by the edge of the water, masks tipped back, breathing tubes removed, and receivers in place. In environment W (Wet), subjects dived to approximately 20 ft., taking with them their formica board and two pencils, and with their receivers in position. Heavy weighting enabled them to sit on the bottom, and the session began after a verbal signal to the surface operator signified their readiness.

Due to a series of technical difficulties, two subjects were eventually dropped from the programme. They were subsequently replaced by another pair who were tested at a freshwater site.

Results

Mean recall scores and standard deviations are shown in Table 1. An analysis of variance showed there to be no significant main effect on recall performance due to either the environment of learning or that of recall. The interaction between

Table 1. *Mean number of words recalled in Expt. I as a function of learning and recall environment*

	Recall environment				
	Dry		Wet		
Learning environment	Mean recall score	S.D.	Mean recall score	S.D.	Total
Dry	13·5	5·8	8·6	(3·0)	22·1
Wet	8·4	3·3	11·4	(5·0)	19·8
Total	21·9	—	20·0	—	—

the effect of learning and recall environment was, however, highly significant ($F = 22{\cdot}0$; d.f. = 1, 12; $P = < 0{\cdot}001$). Thus the effect on recall of the environment of recall depended on the environment of original learning. No other interactions proved significant. Finally, to look in more specific detail at the data, a Wilcoxon matched-pairs, signed-ranks test showed that, for learning in environment D, recall in environment D was better than recall in environment W ($P < 0{\cdot}005$) while for learning in environment W, recall in W was better than recall in D ($P < 0{\cdot}025$). There was no significant difference in recall between conditions DD and WW, nor between conditions DW and WD.

Discussion

The results of Expt. I are clearly in line with the context-dependent memory hypothesis: what was learned under water was best recalled under water and vice versa. Before accepting a context-dependent interpretation, however, some possible shortcomings of the experiment should be considered. The divers were on a pleasure-diving holiday at the time of the experiment, and it is entirely due to their good will in agreeing to participate, and tolerance in accepting the subsequent demands of the experiment that the latter was completed at all. Nevertheless, since the divers were in no way committed to the experiment, some limitations on what could reasonably be done were experienced. Thus, there was no control over the time of day at which subjects were tested. In addition the experiment had to be run each day at the diving site chosen by the subjects, rather than at a constant location. Diving expeditions are notoriously difficult to organize and run smoothly; in an equipment-intensive operation which depends strongly on such local conditions as weather, fitness, etc., something will usually disrupt planned routine. Some divers may not, for medical reasons, dive each day. A dive may have to be aborted due to equipment failure, and so on. These problems were experienced to some degree, as was the unexpected. One diver was nearly run over during an underwater experimental session by an ex-army, amphibious DUKW. Thus it proved impossible to complete the session in four successive days, and the time between sessions varied both within and between subjects. None the less, under realistic open water conditions, and even subject to the above problems, a highly significant interaction between the environment of learning and that of recall emerged.

Before concluding that this result reflects the effect of context-dependent memory three alternative explanations should be considered. The first of these assumes differential cheating between the four conditions, with subjects in the WW group copying items down before the recall signal. Unfortunately, since it was not practicable to combine monitoring of both the subject and the shore station, this is a logical possibility. It is, however, unlikely for the following reasons; (1) had this been the case, higher level of WW performance might have been expected unless subjects were cheating in a particularly restrained manner; (2) it would be difficult to cheat without the awareness and connivance of one's diving partner; and (3) this particular group of subjects had virtually all had previous involvement in an underwater research project of their own, several were in fact research scientists, and social pressures against cheating would be considerable. Nevertheless, differential, restrained collaborative cheating remains a possible explanation. The second alternative explanation assumes that subjects rehearse during the unfilled interval between presentation and test, the degree of rehearsal being greater in the WW and DD conditions than in WD and DW, where subjects might be expected to be distracted by the procedure of getting into or out of the water. Thirdly, there has been a suggestion (Strand, 1970) that context-dependent effects may not be due to environmental change *per se*, but instead to disruption caused by taking the subject from one environment to the other in the context-change conditions. Thus further discussion of the apparent context-dependent effect found here will be postponed until after the report of the next experiment.

EXPERIMENT II

This tests both the differential rehearsal and disruption hypotheses by comparing the standard DD condition used in Expt. I with a modified DD condition in which subjects are required to enter the water and dive during the 4 min. delay between presentation and the subsequent dry test. Both the differential rehearsal and disruption hypotheses should predict poorer retention in this condition, whereas a context-dependent hypothesis would not predict a decrement, since both learning and recall occur in the same environment.

Method

Subjects

Sixteen members of the Scottish Sub-Aqua Club served as voluntary subjects.

Apparatus

Subjects when in the water breathed using a snorkel tube, since this was administratively more convenient than SCUBA equipment. For the limited diving involved the two types of equipment may be regarded as equivalent. All other apparatus including word lists was the same as for Expt. I.

Procedure

As before, stimuli and instructions were all presented via tape. In condition *n* (non-disrupted), tapes employing the relevant lists under condition DD for Expt. I were used. For condition *d* (disrupted), the disruption instruction (to enter the water, swim a short distance, dive to approximately 20 ft., and return to the original position) was dubbed on to copies of the above tapes.

Subjects were assigned randomly to four groups as they volunteered. Prior to the experimental session, subjects practised the task and were then tested at an open water diving site near Oban, Scotland, immediately on their return from their scheduled day's dive.

A within-subject design was adopted. Groups 1 and 2 experienced condition *n* on the first session while groups 3 and 4 were presented first with condition *d*. List *A* occurred on the first session and List *B* on the second, for groups 1 and 3; List *B* was used on the first session and List *A* on the second, for groups 2 and 4. The two sessions were separated by approximately 24 hr.

Insufficient volunteers were found at Oban to complete the design. Consequently, a further three subjects from Stirling University were tested on a subsequent occasion at a freshwater site.

Results

In condition *n*, the mean number of words recalled was 8·44 ($\sigma = 4{\cdot}1$), while condition *d* produced a mean recall score of 8·69 ($\sigma = 3{\cdot}2$). The marginal difference, in the opposite direction to that predicted by the disruption hypothesis, did not approach significance.

DISCUSSION

Scores obtained in Expt. II were lower than those obtained during the comparable DD conditions of Expt. I. Two possible reasons for this arise. Considerable background noise from other nearby diving groups persisted at a relatively constant rate throughout the experiment; this was not generally the case in Expt. I. Furthermore, most of the volunteers, unlike Expt. I subjects (largely medical students), were unfamiliar with testing situations and experimental procedures. Whatever the reason, the criticism might arise that possible effects of disruption might be masked by a floor effect. However, the two mean scores, both between 8 and 9, represent about 25 per cent recall, a reasonable level at which one might expect an effect to be measurable if it exists. Secondly, if 'high scorers' are identified as those subjects who scored higher than the mean under either condition, it is found that this sub-group recalled a mean of 10·1 words in condition *n* and 10·4 in condition *d*, suggesting that the absence of a disruption effect characterizes both high and low scores. Thus it can be concluded that under open water conditions similar to those for Expt. I, disruption was not a significant factor.

CONCLUSIONS

The effect of the environment or context of recall on performance depends upon the environment of learning. Recall is better if the environment of original learning is reinstated. This is unlikely to be due to disruption, and one can be reasonably confident that it is a truly context-dependent phenomenon. The results were obtained under real conditions, away from the laboratory, and using natural environments. The open water setting for the project limited the design such that two separate experiments were needed. However, within the limitations thus imposed, it appears that the phenomenon of context-dependent memory not only exists, but is robust enough to affect normal behaviour and performance away from the laboratory.

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REFERENCES

BILODEAU, I. M. & SCHLOSBERG, H. (1951). Similarity in stimulating conditions as a variable in retroactive inhibition. *J. exp. Psychol.* **41**, 119–204.

BRUNSWIK, E. (1956). *Perception and the Representative Design of Psychological Experiments.* Berkeley: University of California Press.

DALLETT, K. & WILCOX, S. G. (1968). Contextual stimuli and proactive inhibition. *J. exp. Psychol.* **78**, 475–480.

DENNIS, W. (ed.) (1948). *Readings in the History of Psychology.* New York: Appleton-Century-Crofts.

EGSTROM, G. H., WELTMAN, G., BADDELEY, A. D., CUCCARO, W. J. & WILLIS, M. A. (1972). Underwater work performance and work tolerance. Report no. 51, Bio-Technology Laboratory, University of California, Los Angeles.

FARNSWORTH, P. R. (1934). Examinations in familiar and unfamiliar surroundings. *J. soc. Psychol.* **5**, 128–129.

GREENSPOON, J. & RANYARD, R. (1957). Stimulus conditions and retroactive inhibition. *J. exp. Psychol.* **53**, 55–59.

JENSEN, L. C., HARRIS, K. & ANDERSON, D. C. (1971). Retention following a change in ambient contextual stimuli for six age groups. *Dev. Psychol.* **4**, 394–399.

PESSIN, J. (1932). The effect of similar and dissimilar conditions upon learning and relearning. *J. exp. Psychol.* **15**, 427–435.

RAND, G. & WAPNER, S. (1967). Postural status as a factor in memory. *J. verb. Learn. verb. Behav.* **6**, 268–271.

STRAND, B. Z. (1970). Change of context and retroactive inhibition. *J. verb. Learn. verb. Behav.* **9**, 202–206.

ZENTALL, T. R. (1970). Effects of context change on forgetting in rats. *J. exp. Psychol.* **86**, 440–448.

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IMAGERY AND DEEP STRUCTURE IN THE RECALL OF ENGLISH NOMINALIZATIONS

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Previous experiments on the effects of imageability and deep-structure complexity upon the recall of nominalizations are criticized for employing inappropriate statistical techniques and unsuitable experimental material. Two experiments investigated the immediate free recall of homogeneous lists of items, and found that deep-structure complexity had little effect when imageability was controlled. In a third experiment, subjects were cued with either the participle or the noun belonging to a nominalization, but deep-structure complexity still had no effect. In all three experiments, however, imageability was an important determiner of recall performance.

Rohrman (1968) proposed that the memorability of phrases and sentences could be predicted by the complexity of the representation in deep structure which is ascribed to them by transformational grammar. In order to test this suggestion, he investigated free-recall memory for English subject nominalizations, such as 'growling lions', and object nominalizations, such as 'raising flowers'. According to transformational grammar, the two types of phrase are identical in the form of their surface structures, but differ in the form of their deep structures. 'Growling lions' has a deep structure of the form 'lions growl'; this contains two elements, which have to be permuted in order to attain the appropriate surface structure. 'Raising flowers' has a deep structure of the form 'PRO raises flowers'; this contains three elements, and the dummy marker PRO must be deleted in order to attain the appropriate surface structure. Rohrman carried out three experiments, and in each case found that subject nominalizations were easier to remember than object nominalizations. This can be explained either in terms of the number of elements in the deep-structure representation of a phrase, or in terms of its transformational history. Therefore he carried out another experiment using nominalizations that differed in their transformational histories but were equivalent in the complexity of their deep structures. This produced no difference in recall, which suggested that the previous results should be ascribed to differences in deep-structure complexity rather than to differences in the type of transformation involved in the derivation of a particular nominalization.

Paivio (1971*a*) described Rohrman's experiments as 'the only uncontradicted evidence to date that a useful conception of memory can be derived from transformational grammar'. In pursuing the general idea that the memorability of verbal material can be predicted by its imageability, he suggested that deep-structure complexity might correlate negatively with the imageability of Rohrman's nominalizations, and that the latter might turn out to be the better predictor of recall. He showed that in the case of the pool of 160 nominalizations used by Rohrman (1968), the subject nominalizations were rated as significantly more imageable than the object nominalizations. In three experiments using different sets of material, Paivio found that the imageability of a nominalization correlated significantly with its recall, but that subject and object nominalizations did not differ in their memorability when imageability was controlled. In each experiment there was a significant interaction between the two variables, but these interactions were inconsistent across experiments. Paivio concluded that an imagery interpretation of the recall of nominalizations is plausible whereas one based upon deep-structure complexity is not.

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However, all of these results are relative to the particular sets of material used. Neither Rohrman nor Paivio employed the correct statistical procedures for generalizing across both subjects and material. Clark (1973) has criticized past research in general for failing to employ error terms for testing effects of stimulus attributes which take into account both sources of variation. Before continuing any theoretical discussion of memory for English nominalizations, it is clearly necessary to establish the statistical reliability of the effects which have been reported. The experiments described here compare the effects of imageability and deep-structure complexity, using the kind of analysis which Clark recommends. The first experiment investigated the immediate free recall of homogeneous lists of ten items. The material is that employed in Expt. I of Paivio (1971*a*).

EXPERIMENT I

Method

Subjects

Fifty undergraduates at the University of Sussex were tested.

Materials

The four sets of nominalizations shown in Table 1 of Paivio (1971*a*) were presented as separate lists. These varied orthogonally: high versus low imagery, and subject versus object nominalizations. ('Watching movies' was replaced by 'watching films'.)

Imagery ratings

The material was given in imageability rating questionnaires of the sort described by Paivio (1971*a*) to ten undergraduates at the University of Sussex. The mean ratings for the high imagery lists were 5·68 and 5·75, for the subject and object nominalizations, respectively. The means for the low imagery lists were 3·62 and 2·88. An analysis of variance was carried out on each rater's mean rating to each of the four lists, and this yielded only an effect of imagery ($F = 148{\cdot}77$; d.f. = 1, 27; $P < 0{\cdot}001$), i.e. the original design was recovered exactly, even when English raters were used.

Procedure

The remaining 40 subjects were given standard free-recall instructions, and the four lists were read out to them by the experimenter at a rate of one item every 5 sec. After each list, they were allowed 40 sec. for oral recall. The order of the items within each list was determined by a Latin square, as was the order of the four lists to each subject. The subjects were tested individually.

Results

The mean numbers of subject nominalizations correctly recalled were: High I, 6·4; Low I, 5·3. The mean numbers of object nominalizations correctly recalled were: High I, 6·0; Low I, 3·8. Ignoring the order of presentation of the lists, the Latin-square design of this experiment yields complete information concerning the interactions between lists and serial position, and partial information concerning that between items and serial position; it is related to Plan 12 of Winer (1962, pp. 571–4). An analysis of variance was carried out on each subject's score on each item. There were significant effects of serial position ($F = 18{\cdot}60$; d.f. = 9, 288; $P < 0{\cdot}001$), imagery ($F = 14{\cdot}29$; d.f. = 1, 36; $P < 0{\cdot}001$), and nominalization type ($F = 4{\cdot}84$; d.f. = 1, 36; $P < 0{\cdot}05$). There was also a significant interaction between serial position and imagery ($F = 2{\cdot}14$; d.f. = 9, 288; $P < 0{\cdot}05$), such that the effect of imagery was confined to the first eight serial positions. There were no further significant effects; in particular, the interaction between imagery and nominalization type was not significant ($F = 1{\cdot}54$; d.f. = 1, 36; $P > 0{\cdot}2$).

DISCUSSION

This experiment has supported Paivio's (1971*a*) position, in that it has found a substantial effect of rated imageability on the recall of English nominalizations. It has also produced a small, yet statistically significant effect of nominalization type. Even if this represented a genuine effect of deep-structure complexity, one would still have to conclude that this was not an important factor in determining recall performance. However, it might be caused by other factors. For example, although the imagery ratings collected from English subjects do not deviate significantly from the original design, they do tend to reflect the results of the recall tests. It might therefore be argued that the effect of nominalization type was caused by a failure to control exactly for imageability. A more serious criticism concerns the nature of the experimental material. Many of the phrases selected by Paivio (1971*a*) are ambiguous in one respect or another. Some are ambiguous between subject and object forms: the material of Expt. I includes 'grinding lenses' and 'inflating balloons'. Others are ambiguous between two subject nominalizations: examples include 'dancing girls' (i.e. 'girls who are dancing' or 'girls who are employed to dance') and 'thinking scientists'. Two phrases include typing errors in transcribing from Rohrman's original pool of material: 'downing fools' for 'drowning fools' (which is in any case ambiguous), and 'lurching drinks' for 'lurching drunks' (Rohrman, in preparation). Phrases used in the later experiments reported by Paivio (1971*a*) are sometimes bizarre or anomalous; they include 'solving situations' and 'vanishing butterflies'. These problems may be avoided by using a more carefully selected set of material. Expt. II therefore consists of a replication of Expt. I, using new lists of phrases.

EXPERIMENT II

Method

Subjects

Forty-eight pupils at a local grammar school and fifty undergraduates at the University of Sussex were tested.

Materials

The pool of 360 nominalizations employed by Paivio (1971*a*) was reduced to 244 items by excluding any phrase which, in the opinion of either the author or another judge, was clearly ambiguous or anomalous. The remaining phrases were then given in imageability rating

Table 1. *Nominalizations used in Expt. II*

Subject	I	Object	I
		High imagery nominalizations	
Barking dogs	5·81	Mashing potatoes	5·79
Roaring crowds	5·71	Polishing shoes	5·69
Frightening cliffs	5·44	Watching films	5·44
Snarling tigers	5·40	Losing money	5·42
Thrilling sensations	5·35	Digging holes	5·35
Glowing candles	5·31	Mowing lawns	5·31
Arriving ambulances	5·19	Mopping floors	5·10
Fattening meals	5·06	Painting gates	5·04
Flowing streams	5·06	Plotting graphs	5·04
Sleeping babies	4·94	Selecting gifts	4·94
Mean	5·33	Mean	5·31
		Low imagery nominalizations	
Astonishing beliefs	3·19	Paroling convicts	3·23
Intriguing results	3·10	Trapping wolves	3·21
Competing manufacturers	3·08	Shirking obligations	3·08
Puzzling laws	2·94	Realizing ambitions	2·94
Fading opportunities	2·69	Foretelling outcomes	2·65
Existing situations	2·60	Stealing flags	2·63
Emerging nations	2·58	Editing books	2·52
Worsening memory	2·50	Proposing investigations	2·52
Shocking satire	2·33	Adopting methods	2·35
Fleeting freedom	2·29	Formulating opinions	2·35
Mean	2·73	Mean	2·75

questionnaires to the 48 pupils. All of these subjects were aged between 16 and 18, and were in their penultimate year at school before entering university. They were tested as a single group. On the basis of the results of this study, a new set of material was selected; this is shown in Table 1.

Imagery ratings

This material was included in the imageability rating questionnaires described in Expt. I. The mean ratings for the high imagery lists were 5·87 and 5·32, for subject and object nominalizations, respectively. The means for the low imagery lists were 2·52 and 2·91. An analysis of variance was carried out on each rater's mean rating to each list, and this yielded only an effect of imagery ($F = 157{\cdot}27$; d.f. = 1, 27; $P < 0{\cdot}001$); again, the original design was recovered exactly.

Procedure

The procedure of Expt. I was followed exactly.

Results and discussion

The mean numbers of subject nominalizations correctly recalled were: High I, 5·4; Low I, 3·4. The mean numbers of object nominalizations correctly recalled were: High I, 5·5, Low I, 3·4. The analysis of results was identical to that of Expt. I. This yielded significant effects of serial position ($F = 29{\cdot}68$; d.f. = 9, 288; $P < 0{\cdot}001$) and imagery ($F = 18{\cdot}69$; d.f. = 1, 36; $P < 0{\cdot}001$). Neither the effect of nominalization type nor its interaction with imagery was significant ($F < 1$).

Using a more rigorous design and more carefully selected material than in Paivio's experiments, imageability has been found to be an important predictor of recall in the case of English nominalizations, whereas their deep structure has been found to be quite unimportant. The results on subject and object nominalizations are almost exactly the same; there is no interaction of the sort found by Paivio (1971*a*).

EXPERIMENT III

The possibility remains that free recall is not suitable for detecting effects of deep-structure complexity. Experiments by Blumenthal (1967) and by Blumenthal & Boakes (1967) suggest that deep-structure relationships should be investigated using prompted recall. They compared full passive sentences, such as 'The suit was made by John', and truncated passive sentences, such as 'The suit was made by hand'. Such sentences receive the same constituent analysis in transformational grammar; in particular, the logical subject of the first sentence has the same role in surface structure as the adverbial in the second kind. However, logical subjects in the first kind of sentence acted as more efficient prompts to the recall of the whole sentence than the nouns included in the adverbial phrases. This could not be explained in terms of the memorability of the sentences themselves, as a control group prompted with the logical objects of the sentences showed no difference between the two types. Second, in the sentence, 'John is easy to please', the noun serves as the logical object of the verb 'please', and the adjective is taken to modify the sentence, 'Someone pleases John'. However, in the sentence, 'John is eager to please', the noun serves as the logical subject of the verb, while the adjective merely modifies the noun. It was found that sentence-modifiers were more efficient than noun-modifiers, and that logical subjects were more efficient than logical objects, as prompts to the recall of the whole sentences. The next experiment examined whether relational aspects of the deep structure of nominalizations can be revealed in prompted recall.

Method

Subjects

The subjects were 122 pupils at a local grammar school, tested as a group. They were at the same age and educational level when tested as the 48 subjects who carried out the rating task described in Expt. II.

Materials

The items used in Expt. II were randomized, with the constraint that each successive set of four items was to contain one phrase of each type. Ten other phrases were added, five as primacy fillers at the beginning of the list, and five as recency fillers at the end of the list.

Procedure

The subjects were instructed to try to remember the list of 50 nominalizations, which was then read out at a rate of one item every 5 sec. Following the presentation of the list, 33 subjects were given standard free-recall instructions; 46 subjects were prompted with the participles of the 40 critical phrases, and were instructed to recall the corresponding nouns; and 43 subjects were prompted with the nouns of the 40 critical phrases, and were instructed to recall the corresponding participles. Guessing was encouraged, responses were written, and 5 min. was allowed for recall.

Results

Subjects were discarded at random from the two prompted-recall groups to yield three equal groups of 33 subjects. The mean numbers of nominalizations (ignoring

Table 2. *Mean items recalled in Expt. III, by imagery, nominalization type and mode of recall*

	Subject nominalizations		Object nominalizations	
	High I	Low I	High I	Low I
Free recall	2·1	1·3	2·0	0·7
Participle prompt	4·4	1·8	6·0	2·1
Noun prompt	5·1	1·7	5·2	1·8

the primacy and recency fillers) given correctly under each of the three conditions are shown in Table 2. An analysis of variance was carried out on each subject's performance on each item. The between-groups variation was partitioned into that between the two prompted-recall groups, and that between prompted recall and free recall. The analysis yielded a significant difference between the two groups with prompted recall and the group with free recall (quasi $F = 43{\cdot}66$; d.f. = 1, 100; $P < 0{\cdot}001$), a significant effect of imagery (quasi $F = 32{\cdot}31$; d.f. = 1, 43; $P < 0{\cdot}001$), and a significant interaction between these two effects (quasi $F = 19{\cdot}67$; d.f. = 1, 70; $P < 0{\cdot}001$). The effect of imagery was significant in the case of prompted recall (quasi $F = 57{\cdot}80$; d.f. = 1, 96; $P < 0{\cdot}001$), but not in the case of free recall (quasi $F = 2{\cdot}74$; d.f. = 1, 96; $P > 0{\cdot}10$). No other term in the analysis of variance was significant.

Discussion

The interaction between type of test and imagery is at least in part caused by the floor effect in free recall. Further experiments would be necessary to substantiate this as the sole cause, but this is largely irrelevant to the present concern. The results of this experiment include the important finding that deep-structure relationships may not be effective in memory tasks even when the mode of testing might most favour them. Of course, the possibility still exists that effects of deep-structure relationships might be favoured by other sorts of *material*, complete sentences, for example, as in Blumenthal's studies. However, since a subject nominalization and an object nominalization cannot both appear in any but the most vacuous sentence-frames (e.g. 'They are...'), it would not be possible to employ the present material to study this possibility while controlling for other properties of sentences. Nevertheless, the small and non-significant effects of nominalization type in the last experiment are once again in marked contrast to the large effects of imageability. One result which is curious is the failure to find any difference between the two prompted-recall tests. Paivio (1963) found that with adjective–noun paired associates performance was superior when the noun was the stimulus to when the adjective was the stimulus. He has recently suggested in personal communication that this effect might be caused by the differential imageability of adjectives and nouns rather than by any genuine syntactic difference. In that case, it would follow that for pairs which were homogeneous with respect to imageability the syntactic category of the members of the pairs (here, participle versus noun) would be unimportant in determining their efficiency as prompts. However, not all the available evidence is consistent with this view (Paivio, 1971*b*, pp. 272–6).

GENERAL DISCUSSION

There are good linguistic reasons for making a distinction between subject and object nominalizations. In terms of deep structure, they can be distinguished by the number of elements presumed to be present in the base representation, and the number and nature of the transformations required to produce acceptable surface structures. In terms of semantics or of case grammar, they can be distinguished in terms of the logical relationships between the components explicitly specified: subject nominalizations usually involve the relationship agent–action, while object nominalizations involve the relationship action–object. However, the experiments reported here suggest overwhelmingly that this distinction has little effect upon a subject's ability to recall such material. This is true even when one of the components is presented as a prompt to the recall of the complete phrase.

On the other hand, all of the experiments reported here support the contention that the imageability of material is a good predictor of its memorability. Their results recommend the development of an imagery interpretation of memory for nominalizations, rather than one based upon deep-structure complexity. While this conclusion is identical to that reached by Paivio (1971*a*), his experiments have been criticized in two fundamental respects, First, his material was not suitable for testing the relevant hypotheses, as it included many anomalies and ambiguities. Second, his statistical techniques did not even address themselves to those hypotheses, since they allowed one at most to come to certain conclusions about the particular material employed in his experiments. Psycholinguistic research has always been aware of the problems involved in selecting experimental material, but it must also pay due regard to the appropriateness of its statistical techniques.

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REFERENCES

BLUMENTHAL, A. L. (1967). Prompted recall of sentences. *J. verb. Learn. verb. Behav.* **6**, 203–206.

BLUMENTHAL, A. L. & BOAKES, R. A. (1967). Prompted recall of sentences. *J. verb. Learn. verb. Behav.* **6**, 674–676.

CLARK, H. H. (1973). The language-as-fixed-effect fallacy: a critique of language statistics in psychological research. *J. verb. Learn. verb. Behav.* **12**, 335–359.

PAIVIO, A. (1963). Learning of adjective–noun paired associates as a function of adjective–noun word order and noun abstractness. *Can. J. Psychol.* **17**, 370–379.

PAIVIO, A. (1971*a*). Imagery and deep structure in the recall of English nominalizations. *J. verb. Learn. verb. Behav.* **10**, 1–12.

PAIVIO, A. (1971*b*). *Imagery and Verbal Processes*. New York: Holt, Rinehart & Winston.

ROHRMAN, N. L. (1968). The role of syntactic structure in the recall of English nominalizations. *J. verb. Learn. verb. Behav.* **7**, 904–912.

WINER, B. J. (1962). *Statistical Principles in Experimental Design*. New York: McGraw-Hill.

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VISUAL IMAGERY AND THE DISCRIMINATION OF DIFFERENCES BETWEEN ALTERED PICTURES SIMULTANEOUSLY AND SUCCESSIVELY PRESENTED

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Ten 'good' visual imagers and ten 'poor' imagers, representing the upper and lower halves of an unselected sample of adult volunteers, were given a task of detecting differences between altered pictures presented in pairs, either simultaneously or successively. Performance was measured by reaction time. 'Good' imagers reacted faster than 'poor' imagers, and the mode of presentation (simultaneous *v.* successive) did not affect their performances. 'Poor' imagers, on the other hand, reacted significantly slower when the pictures compared were presented successively. The results suggest that self-rated visual imagery can be used to predict the successful recall of visual information required in a discrimination task.

Although the enhancing effect of imagery on memory has been subjected to rather extensive investigation (e.g. Bower, 1970; Bugelski, 1970; Neisser, 1970; Paivio, 1969, 1970), the relation between vividness of imagery and its possible effective use in recall remains largely unexplored. This apparent neglect may be attributed at least in part to Neisser's (1970) conclusion, based on an earlier study (Sheehan & Neisser, 1969), that subjectively reported vividness of imagery has little if any effect on the recall of visual stimuli.

Neisser's conclusion was challenged by Marks (1973) on two grounds: first, because of shortcomings in the Betts (1909) scale used by Sheehan & Neisser (1969), and second, because of the rather sterile geometric designs used in their study of recall.

A shortcoming of the Betts scale is its reliance on a large number of unrelated stimuli from many different sensory modalities, a feature that makes the many ratings of imagery a mechanical and uninvolving task. Marks (1973) developed his own vividness of imagery scale, and used more interesting items in his test of visual recall. His Vividness of Visual Imagery Questionnaire (VVIQ) contains only 16 items, which require the subject to rate his imagery on four different aspects of visual scenes reflecting the subject's own experience (a relative or friend, a rising sun, a shop to which he often goes, and a country scene). The subject considers his 'picture' carefully, and replies by ratings of vividness on four aspects of it, hence making only 16 ratings altogether. For example, for the relative or friend he must consider the contour of the face, head, shoulders and body; next, characteristic poses; third, the precise carriage; and finally, the different colours worn in some familiar clothes. The scene is not a static one, but requires some manipulation of the image. The questionnaire proved sufficiently reliable for group studies (test–retest reliability coefficient 0·74, with $n = 68$). The validity of the questionnaire was supported by demonstrating that those with better imagery were more successful in their recall of meaningful pictures arranged in complex patterns. The recall questions required not only descriptive characteristics ('What was the time on the clock?') but location ('What was directly below the suitcase?').

The visual imagery measure developed by Marks is a rather global one, and not too much should be made of its emphasis upon vividness. Although called a scale of 'vividness', it may reflect other dimensions of imagery as well, such as imagery controllability (Gordon, 1959; Richardson, 1969). The rating scales used emphasized vividness and clarity, but the tasks required the subject to alter his perspective while imaging (seeing the store window from across the street, and a window display close up). We have not been concerned about the precise dimensions of imagery involved, but wished to determine whether it satisfactorily distinguished between 'good' and 'poor' visual imagers in some meaningful way. Marks found that the test did indeed predict visual recall, and our results, apart from their substantive interest, may be considered an additional validation of Marks' questionnaire, showing that indeed a subjective report of visual imagery is more useful in studies of recall than Neisser (1970) was led to believe.

The purpose of the present investigation is to explore the extent to which a subject's ability to conjure up an image of a visual stimulus can substitute for the presence of that stimulus when a comparison with another is required. More specifically, on the basis of Marks' findings, we hypothesized that, in a task requiring a discrimination between two pictures, one altered from the other, good visual imagery will compensate for the absence of one of the pictures to be compared. Hence a simultaneous presentation should have less advantage over a successive comparison for 'good' than for 'poor' imagers.

METHOD

Subjects

Twenty adult volunteers, primarily graduate students (10 male, 10 female), served as subjects in this experiment.

Stimulus materials

The Vividness of Visual Imagery Questionnaire (VVIQ) developed by Marks (1973) was used to assess vividness of imagery.

Thirty pairs of pictures selected from the Meier Art Judgement Test (Meier, 1940) provided the discrimination task. Each pair consists of one black and white copy of an original art work and a slightly modified version of that work prepared in the same style. The 30 pairs were divided into two sets, 15 pairs in each, on the basis of judged similarity in the nature of the paired art works and the kinds of differences introduced in the modified versions. The order of the stimuli within each set was randomized.

Procedure

The subjects were tested individually. After completing the 16-item VVIQ, each was presented with the experimental manipulation, which consisted of two different procedures for detecting the difference between the original and the modified picture presented in each stimulus pair. The two procedures were:

1. *Simultaneous presentation*: The subject was asked to look at the two pictures on the card taken from the Meier test, then point as fast as he could to the part of the picture in which the two art works differed. He was then asked to state what the difference was.

2. *Successive presentation*: The subject was allowed to inspect one of the two pictures printed on the test card for 30 sec. He was then asked to close his eyes for 20 sec, after which he was exposed to the second picture in the pair. His task, again, was to point as fast as he could to the part in which the picture differed from the previously presented picture, and to tell what the difference was.

The order of presentation of the two procedures was counterbalanced, as well as the set of test items assigned to a given procedure.

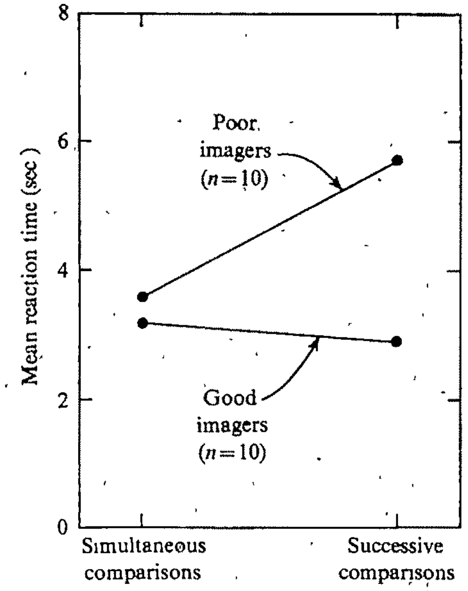

Fig. 1

The time it took for a subject to detect the difference between the two pictures in a pair was measured by a stop-watch. It proved unnecessary to correct for errors because, instead of making errors, subjects increased their searching times to locate a true difference. The differences, once detected, were in all cases conspicuous ones.

RESULTS

The 20 subjects were divided by the median of their VVIQ scores into 10 'good' imagers (mean = 70·6, S.D. = 3·5) and 10 'poor' imagers (mean = 50·3, S.D. = 12·5). The mean reaction times for each group of subjects under each experimental condition are presented in Fig. 1.

The results were subjected to a 2 ('good' *v.* 'poor' imagers) × 2 (successive *v.* simultaneous presentation) analysis of variance with repeated measures on one factor. This analysis revealed a significant 'goodness of imagery' effect ($F = 6{\cdot}01$; d.f. = 1, 18; $P < 0{\cdot}01$), as well as a significant mode of presentation effect ($F = 4{\cdot}57$; d.f. = 1, 18; $P < 0{\cdot}05$) and a significant interaction ($F = 7{\cdot}67$; d.f. = 1, 18; $P < 0{\cdot}01$). A comparison between the means indicated that the 'poor' imagers reacted significantly slower to the successive as compared to the simultaneous mode of presentation, whereas the 'good' imagers were not affected by presentation mode. These results are in agreement with the experimental hypothesis.

In order to assess the magnitude of the relationship between imagery and performance on the visual test, the mean reaction time for the total 30 test items was computed for each subject. The correlation between these scores and the scores on the Vividness of Visual Imagery Questionnaire was $-0{\cdot}74$ ($P < 0{\cdot}01$), indicating that the higher the imagery score, the shorter the overall reaction time for the visual test. This finding provides additional support both for the role of imagery in facilitating the processing of visual information, and for the usefulness of the Marks test in assessing visual imagery.

DISCUSSION

The present investigation shows that people who are able to conjure up good images in their 'mind's eye' can, and apparently do, use this ability in a task requiring them to compare a 'mental' image with a 'real' picture. This finding is congruent with those reported by Marks (1973). Further more, it contradicts Neisser's (1970) dismissal of the 'introspective' type of imagery questionnaires. In the present investigation, such a questionnaire proved to be highly predictive of what is agreed to be a rigorous and 'objective' performance measure (i.e. reaction time).

Because all of the subjects could make the discriminations required, all had sufficient memory for detail in the original picture to make the necessary judgements in the successive comparisons. The differences in reaction time between the 'good' and 'poor' imagers suggest, however, that they used the information in different ways. The 'poor' imagers recalled details of the original picture, and then searched the new picture for differences: ('There was a boat in the foreground of the original picture; it has been moved'). This process of item-by-item search took more time than the method used by the 'good' imagers. They commonly reported that the difference 'popped out', as though the original picture was a matrix that could be placed over the second one for the detection of difference. This method required less time. The method (or a closely related one) was apparently available to the 'poor' imagers in the simultaneous tasks, where rapid looking back and forth might serve the same purpose of matrix comparison. Individual differences in the ability to form visual images, and to manipulate them, may turn out to be a component of more generalized individual differences in modes of information processing, but additional research is necessary before confident assertions can be made. In particular, because some of these abilities are reflected in Marks' Vividness of Imagery Questionnaire, it would be desirable to know the determiners of the subject's response to it, and what are the major dimensions of imagery so reported.

Another avenue of future research may lie in attempts to enhance imagery as an aid to information processing. This investigation was undertaken, in fact, in order to find more satisfactory parameters for assessing imagery in relation to an ongoing research programme in hypnosis being carried on in this laboratory. It has been found that highly hypnotizable subjects tend to have a capacity for 'imaginative involvements' (Hilgard, 1970). Although the Betts (1909) questionnaire was found to correlate only moderately with hypnotizability (Perry, 1973), the present results lead us to expect better results with the type of scale developed by Marks (1973). If this proves to be the case, the possibility also exists of using hypnosis to increase imagery, and to detect these changes through the improved measures.

In summary, the present study encourages further explorations of visual imagery in relation to the processing, recall and utilization of visually perceived information.

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REFERENCES

BETTS, G. H. (1909). The distribution and function of mental imagery. *Columb. Univ. Contrib. Educ.* **26**, 1–99.

BOWER, G. H. (1970). Imagery as a relational organizer in associative learning. *J. verb. Learn. verb. Behav.* **9**, 529–533.

BUGELSKI, B. R. (1970). Words and things and images. *Am. Psychol.* **25**, 1002–1012.

GORDON, R. (1949). An investigation into some of the factors that favour the formation of stereotyped images. *Br. J. Psychol.* **39**, 156–167.

HILGARD, J. R. (1970). *Personality and Hypnosis: a Study of Imaginative Involvement.* Chicago: University of Chicago Press.

MARKS, D. F. (1973). Visual imagery differences in the recall of pictures. *Br. J. Psychol.* **64**, 17–24.

MEIER, N. C. (1940). *The Meier Art Tests: I. Art Judgment.* Iowa City, Iowa: Bureau of Educational Research and Service.

NEISSER, U. (1970). Visual imagery as process and experience. In J. S. Antrobus (ed.), *Cognition and Affect.* Boston: Little, Brown.

PAIVIO, A. (1969). Mental imagery in associative learning and memory. *Psychol. Rev.* **76**, 241–263.

PAIVIO, A. (1970). On the functional significance of imagery. *Psychol. Bull.* **73**, 385–392.

PERRY, C. (1973). Imagery, fantasy, and hypnotic susceptibility: a multidimensional approach. *J. Person. soc. Psychol.* **26**, 208–216.

RICHARDSON, A. (1969). *Mental Imagery.* New York: Springer.

SHEEHAN, P. W. & NEISSER, U. (1969). Some variables affecting the vividness of imagery in recall. *Br. J. Psychol.* **60**, 71–80.

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THE SUBLIMINAL PERCEPTION OF MOVEMENT AND THE 'SUPPRESSION' IN BINOCULAR RIVALRY

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An analogy is drawn between the perceptual limitation that characterizes the dichotic listening paradigm and the 'suppression' that occurs in binocular rivalry when different stimuli are presented to the two eyes. An experiment is reported which focuses on the fate of the information residing in a suppressed eye (unattended channel) during binocular rivalry. It is demonstrated that the temporal course of rivalry is sensitive to the presence of a ***subliminal*** moving stimulus within the currently ***suppressed*** field. The effects are seen to confirm a literal interpretation of Levelt's (1966) thesis which relates changes in the 'stimulus strength' of a rivalling field to subsequent changes in the temporal course of the phenomenon. This interpretation is consistent with the hypothesis that, despite phenomenal suppression, a full analysis is undertaken on the currently non-dominant stimulus. The data are related to models of selective attention, and to the notion that there are parallel visual systems.

When different stimuli are presented to the two ears in the dichotic listening paradigm, the limited capacity of the perceptual system is reflected most dramatically in the recipient's lack of awareness for much of the content of the unattended channel (cf. Corteen & Wood, 1972). In the visual domain a similar limitation is reflected in the suppression that occurs in binocular rivalry when different stimuli are presented to the two eyes.

In evaluating the alternative models of selective attention that have emerged from the dichotic listening studies, the fundamental question has concerned the fate of the information in the unattended channel. At what point, or after what sort of analysis is this information discarded? Thus, whilst Broadbent's (1958) model permits an analysis of only the major physical characteristics of the unattended information, just sufficient to direct a filter's subsequent suppression of the material, the Deutsch & Deutsch (1963) model allows for a full analysis. Since these or similar theories may be applied to its visual analogue, it was deemed instructive to ask the same fundamental question with respect to binocular rivalry.

In the experiment to be reported below, an attempt was made to determine whether the perceptual system is capable of discriminating a moving pattern that is presented within the currently non-dominant field in binocular rivalry. More specifically, it was asked whether the introduction of movement into such a field would reduce the amount of time for which it remained non-dominant. Reference to Levelt's (1966) thesis, which relates changes in the 'stimulus strength' of a rivalling field to subsequent changes in the temporal course of the phenomenon, provided more precise predictions. Consistent with his preliminary definition of 'stimulus strength', it was assumed that adding movement to a field would serve to increase that field's strength.

Quite apart from its relevance in this respect, Levelt's thesis has a direct bearing on the experimental problem since his central proposition is that 'the mean duration of the dominance of the stimulus in one eye is independent of the strength of this stimulus;

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the duration is assumed to be dependent only upon the strength of the stimulus in the contralateral eye' (Levelt, 1966, p. 225). A literal interpretation of this suggests, then, that at any instant the probability that a shift in dominance will occur is dependent upon the properties of the currently *non-dominant* stimulus as they are revealed by a *current analysis of the sensory information*. Confirmation of this extension to Levelt's thesis would therefore suggest an explanation of binocular rivalry in line with the Deutsch's treatment of selective attention.

Unfortunately, Levelt's data do not provide this confirmation. His procedure did not eliminate the possibility that the mechanism responsible for the rivalry alternations responded to the properties of the currently non-dominant field by referring to some stored representation (made available during a preceding dominant phase of the field) rather than to the results of a current analysis of the sensory information. This problem was circumvented in the present study by having the introduction of movement to a rivalling field restricted to coincide with *either* its non-dominant or dominant phases.

Finally, it was reasoned that whatever aspect of the visual system is capable of responding to information in the non-dominant field, it is a likely mediator of subliminal perception (cf. Dixon, 1971) since *ipso facto* it can function in the absence of a perceptual (phenomenal) adjunct to the information with which it is dealing. As a test of this, a condition involving the movement of a subliminal stimulus was incorporated in the experiment.

METHOD

Each subject reported the alternations between rivalling red and green fields that were presented to his right and left eyes respectively (cf. Figs. 1, 2). Preliminary experiments demonstrated that providing a difference in overall size of the fields (the red being smaller than the green) served to discourage (i) the two fields from fusing to yield 'cortical yellow', and (ii) the occurrence of a piecemeal form of rivalry in which different, localized parts of the fields behaved independently.

Variation of the stimulus conditions involved changes to the red field. The six trials undertaken by each subject were segregated into two blocks of three, according to whether the stimuli that were superimposed on this field were at a supraliminal or subliminal (as defined below) level. In addition to a trial involving the presentation of a moving pattern (movement condition), each block incorporated a control trial during which the same pattern remained stationary (stationary condition), and a further trial during which no additional stimuli were presented to the red field (blank condition). With the stationary trial always being given between the blank and movement trials within any block, and half the subjects undertaking the subliminal condition first, there were four different arrangements for the trials. An equal number of subjects undertook the trials in each of these orders.

When the pattern was superimposed on the red field, its presentation was made contingent upon the field reported to be currently dominant. For one group of subjects its presentation was restricted to coincide with the dominant phases of the red field (dominant condition); whilst for a second (independent) group, its presentation coincided with this field's non-dominant phases. Independent groups were employed in this way in order to avoid (i) fatiguing subjects, and (ii) the transfer effects that were evident in some preliminary observations employing the same paradigm.

From the data recorded on each trial the number of alternations (changes in dominance from one field to the other) and the average duration of dominance of each field (mean dominance time) could be calculated.

Apparatus

Semi-transparent tracing-film screens were placed in each field of a modified Unis-France stereoscope. Between these and in the right and left eyepieces were placed, respectively, red

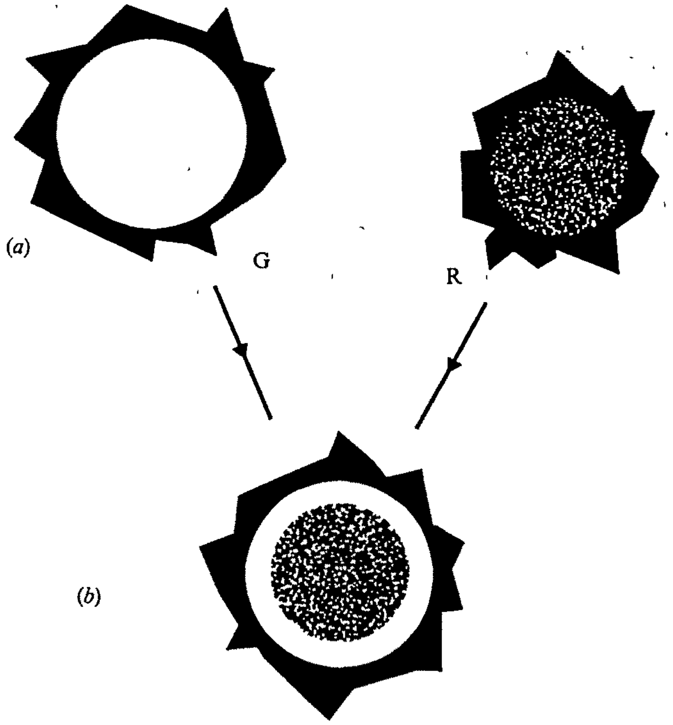

Fig. 1. (a) The rivalling stimuli employed in the experiment. G, The homogeneous green field that was presented to subject's left eye. R, the red field that was presented to the contralateral eye: a sample of the pattern that was projected on to this field being illustrated. Note that upon projection only the lighter parts of this pattern would be visibly superimposed upon the otherwise illuminated, homogeneous red field. In this sense the figure is unrepresentative of the conditions that pertained to the experiment. (b) The relative positioning of the two fields when seen through the stereoscope. The red field is represented as dominant. The difference in size of the two fields provided a green 'rim' which tended not to be suppressed by the red field.

(Ilford: 205) and green (Ilford: 625) colour filters. Whilst the former transmits freely in the 6400 Å to infra-red range (with heavy absorption at other wavelengths) the latter is restricted to between 5100 and 5900 Å. Transilluminated by a small fluorescent lamp (Opal Striplite – 30 V 30 W 221 mm) positioned behind and above the stereoscope, the differently coloured stereoscope fields served as the rivalling images. The red and green fields subtended 11° 18′ and 15° 38′ respectively, and ignoring the small changes that were made in order to counterbalance eye dominance, their luminance measured 1·5 log ft. lamberts.

The moving pattern was projected on to the red field by passing the edge of a rotating transparent disc through the focal plane of a projector (Aldis Tutor 1000; lamp, Philips M 9, 240 V, 300 W, A1/178). Commercial Letratone, pattern LT 100 (a quasi-random pattern, cf. Fig. 1), was applied to the disc. When viewed through the stereoscope, this pattern traversed a linear path, in an upward direction, with a velocity of 20 deg. sec.$^{-1}$ The irregular and fine texture of the pattern ensured that no fluctuation in gross intensity of the red field was confounded with the movement. For the stationary trials the transparent disc was prevented from rotating, whilst for the blank trials, a shutter, placed immediately in front of the projector, was closed.

For the independent control of the gross intensities of the two fields crossed polaroid filters were mounted in front of each eyepiece. Positioned between projector and stereoscope was one of a series of Kodak Wratten neutral density filters. The critical effect of interposing a filter in this way was to decrease the contrast between the pattern and the ground upon which it was projected.

Two microswitches were provided for the subject to report the rivalry alternations. With one

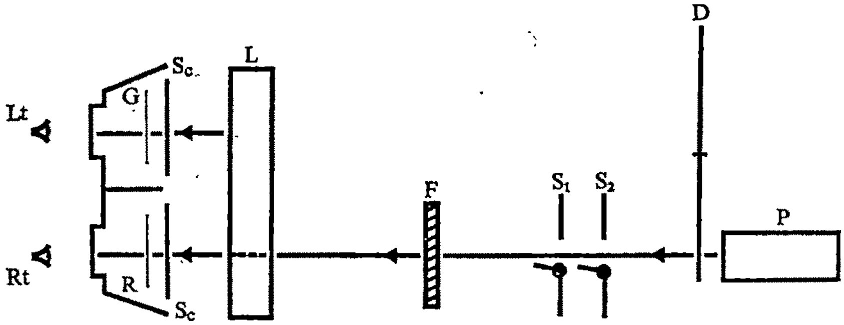

Fig. 2. Schematic diagram of the apparatus. P, Projector; D, rotating transparent disc bearing the pattern illustrated in Fig. 1; S_1 and S_2, shutters; F, neutral density filter; L, fluorescent lamp positioned behind and above the stereoscope; Sc, screens placed in the fields of the stereoscope; G and R, the green and red filters; Lt and Rt, subject's left and right eyes.

switch connected to a camera shutter mounted in front of the projector, the presentation of the pattern could be restricted to the dominant or non-dominant phases of the red field. In addition, each switch was connected to an electronic timer, and an event counter, and to one channel of an Esterline Angus multiple channel pen-recorder.

Subjects

Forty-four subjects took part in the experiment. All were undergraduate students at University College London, though none were students of psychology.

Procedure

The subject first familiarized himself with the stereoscope. the phenomenon of binocular rivalry, and the use of the two microswitches to report the currently dominant stimulus. Next, with the conditions as described above for the movement trials, the absolute awareness threshold for the moving pattern was determined. This threshold was defined as the lowest intensity level (in terms of the value of the neutral density filter placed between the projector and stereoscope) at which the subject ever reported an awareness of being stimulated by the moving pattern during a threshold determination procedure (cf. Dixon, 1971, p. 12). The randomized double-staircase procedure described by Cornsweet (1962) was employed, the size of the 'steps' by which the intensity level was either decreased or increased corresponding to a value of 0·1 for the neutral density filter. In respect to this procedure the subject was given the following instructions: 'If you press the key in front of you and at the same time look into the apparatus you will see that I am presenting you with a moving pattern. What I shall do is sometimes present you with this, and sometimes not. What I want you to do is press the switch when I give you the signal and decide whether or not the moving stimulus is there. You do not actually have to see the whole pattern in motion, if you think you can see any movement, say "yes". The moving pattern is the only thing that will be presented.' Whilst undertaking this task the subject kept both eyes open but was asked to press the key whilst the red field was dominant. The procedure advocated by Cornsweet was adhered to with the exception that a number of 'dummy' trials were included. For these trials one of the shutters in front of the projector was closed, preventing the superimposition of any light on to the red field. After the value of the projected light had levelled out, oscillating between two values over five successive trials in each of the two staircase series (cf. Cornsweet. 1962) the procedure was terminated. A note was made of the lowest intensity level (highest value of neutral density filter) at which the subject reported the moving pattern to be present. For the trials involving the presentation of the pattern (moving or stationary) at a subliminal level, a 0·3 filter was added to the threshold-value filter. A corresponding reduction in the value of the filter ensured the supraliminality of the pattern in the supraliminal trials.

The subject next undertook the six trials of the experiment, each of which lasted 100 sec., in an order that was randomly selected from the four alternatives. The only proviso was that an equal number of subjects should undertake the experiment in each of the possible orders.

Subjects were asked to report the rivalry alternations between the red and green fields by alternately closing the two microswitches. More specifically, they were asked to ignore the green rim which arose from the difference in size of the two fields, and to press the switch in their left hand whenever and for as long as the green field was dominant, and to press the switch in their right hand whenever and for as long as the red field was dominant. Thus, dominance was defined in relation to the red and equivalent area of the green field. In this way, when an image such as is shown in Fig. 1*b* occurred, it was defined as being red dominant, despite the appearance of the green rim. If the colour were to change to any other, subjects were requested to refrain from pressing either key, and to report the event to the experimenter at the end of the trial. The first ten seconds of each trial were allowed for the subject to settle down to observing the rivalry without the need to report the alternations. At the end of this period a short auditory signal indicated the start of the trial proper, and the need to use the two switches. A 1 min. rest-period was given between each trial.

Finally, precautions were taken to detect those subjects for whom the subliminality of the stimuli in that condition was unreliable. To this effect, subjects were approached immediately following the subliminal block of trials with the question: 'you did see the movement, didn't you?' It was intended that any subjects responding positively to this would be rejected from the experiment. Those subjects answering in the negative were nevertheless subjected to an interrogation at the termination of the session. They were asked, 'What do you think the experiment is about – what is it that I am interested in?' 'Did you see anything that you may not have expected to see?' 'On how many trials did I show you the moving pattern?' Subjects' answers to these questions were interpreted in terms of the degree to which they were aware of the true purpose of the experiment, and the extent to which this knowledge derived from an awareness of the supposedly subliminal stimuli.

RESULTS AND ANALYSIS

Though three subjects were unable to complete the experiment due to their extreme eye dominance, none had to be rejected as a result of the interrogation concerning the subliminal condition. That is, no subject reported being aware of the stimuli that were superimposed on the red field in the subliminal trials. Owing to an administrative error, however, the number of subjects undertaking the experiment in the dominant condition was four short of the intended 24. Data from 20 subjects were deemed sufficient.

Separate analyses of variance for repeated measures were applied to the average duration of the dominance phases of the two fields, in both the dominant and suppressed condition. Similar analyses were also undertaken on the number of alternations. The resulting ANOVA tables are presented in Table 1, and the data are tabulated in condensed form in Table 2. The appropriate multiple comparisons, of the Scheffé type, confirmed the significance of a number of effects.

Effects resulting from the presentation of the moving pattern

For the movement condition the multiple-comparison tests (which involved contrasting the movement and stationary trials) failed to assign significance to any of the differences in the dominant condition for either subliminal or supraliminal presentation. This failure is consistent with the interpretation of Levelt's thesis. In contrast, in the suppressed condition both supraliminal and subliminal presentation had significant effects. In the latter case the mean dominance time of the green field was reduced ($F = 5{\cdot}22$; d.f. $= 1, 40$; $P < 0{\cdot}05$), whilst the mean dominance time of the red field remained unaffected. With the increase in the number of alternations in this condition (which, however, failed to attain significance) the results again

Table 1. *Results of analyses of variance applied, in order, to the mean dominance times for green, the mean dominance times for red, and the number of alternations for the dominant and suppressed conditions*

('Order' refers to the different orders of doing the six trials. 'Threshold' refers to the supraliminal/subliminal factor. 'Condition' refers to the blank/stationary/movement factor; cf. text.)

	Dominant (20 subjects)				Suppressed (24 subjects)			
Source	D.F.	S.S.	S.S.	S.S.	D.F.	S.S.	S.S.	S.S.
Between subjects	19	395·05	1198·01	1329·17	23	249·63	411·96	11262·93
Order	3	94·66	102·82	174·73	3	48·38	38·65	2998·41
Between subjects within groups (Error)	16	300 39	1095·19	1154·44	20	201·25	373·31	8264·52
Within subjects	100	488·59	1896·56	2142·69	120	137·98	233·07	9999·32
Threshold	1	56·56	358·63	381·62	1	13·73	0·92	875·11
Threshold × Order	3	1·14	2·74	60·04	3	3·68	2·64	447·91
Threshold × Error	16	114·13	185·41	257·03	20	39·17	50·64	2178·87
Condition	2	17·67	260·73	269·01	2	9·02	13·24	1097·04
Condition × Order	6	3·39	19·41	53·92	6	5·12	17·27	631·29
Condition × Error	32	114·34	372·75	300·76	40	22·86	66·44	2132·40
Threshold × Condition	2	26·96	251·31	400·43	2	8 61	4·08	623·80
Threshold × Condition × Order	6	36·69	11·72	59·71	6	3·73	5·34	406·42
Threshold × Condition × Error	32	117·71	433·86	360·17	40	32·06	72·50	1606·38
Total	119	883·64	3094·57	3471·86	143	387·61	642·50	21262·16

Table 2. *The overall mean dominance times* (*in sec.*) *for the two rivalling fields, and the overall mean number of alternations, that emerged from the blank, stationary and movement conditions* (*cf. text*)

(The stimuli that were superimposed upon one of these fields being at subliminal or supraliminal levels, and made contingent upon the field's dominant or non-dominant (suppressed) status.)

		Dominant			Suppressed		
		Blank	Stationary	Movement	Blank	Stationary	Movement
Overall mean for dominance times of green field	Supraliminal	3·27	5·00	5·61	2·63	1·65	1·80
	Subliminal	3·58	3·11	3·07	2·70	2·91	2·32
Overall mean for dominance times of red field	Supraliminal	3·56	10·42	8·75	4·36	4·17	3·33
	Subliminal	4·07	4·11	4·17	4·39	4·00	3·95
Overall mean for number of alternations	Supraliminal	15·05	8·05	7·95	14·63	20·13	26·42
	Subliminal	13·45	14·20	14·10	15·00	14·75	16·63

conform to the predictions based upon Levelt (1966). For the supraliminal presentation of the moving pattern the predicted increase in alternation rate was observed ($F = 11{\cdot}83$; d.f. = 1, 40; $P < 0{\cdot}05$), though this arose from a reduction in the mean dominance time of the red field ($F = 4{\cdot}68$; d.f. = 1, 40; $P < 0{\cdot}05$) and not the green field as predicted. Though inconsistent with the predictions, subjects' introspective reports did suggest that Levelt's thesis was inapplicable to this condition. Thus, the appearance of a composite image was reported, in which the superimposed *red* pattern was seen to move 'through' or 'above' the contralateral *green* field. Clearly, this makes for difficulties in the interpretation of subjects' responses in this condition, in addition to precluding the application of Levelt's thesis.

Effects resulting from the presentation of the stationary pattern

Multiple comparison tests that involved contrasting the stationary and blank conditions failed to reveal any effects arising from the presentation of the stationary pattern at subliminal levels. In contrast, presentation at a supraliminal level did influence the rivalry alternations. Conforming to the predictions (it is assumed that the pattern added to the stimulus strength of the red field), the effects in the suppressed condition centred on the mean dominance time for the green field. This mean was found to be significantly reduced ($F = 14{\cdot}35$; d.f. = 1, 40; $P < 0{\cdot}05$), causing an increase in the alternation rate ($F = 9{\cdot}04$; d.f. = 1, 40; $P < 0{\cdot}05$). In the dominant condition, and this was not predicted, the mean times of dominance of both the green and red fields increased ($F = 8{\cdot}13$; d.f. = 1, 32; $P < 0{\cdot}01$; and $F = 34{\cdot}7$; d.f. = 1, 32; $P < 0{\cdot}05$, respectively), giving rise to a reduction in the alternation rate ($F = 42{\cdot}3$; d.f. = 1, 32; $P < 0{\cdot}05$).

Due to the confounding of effects arising from the increase in luminance and from the pattern *per se* the results from this section should not be given too much weight. The results of a stochastic analysis of the rivalry alternations in the blank condition (the purpose served by the inclusion of this condition) are to appear in a separate report.

DISCUSSION

With regard to the conditions of central interest, involving the subliminal presentation of the moving pattern, the propositions outlined in the introduction were supported.

(*a*) Movement can be discriminated under conditions that prevent the subject from being aware of the stimulus. In the suppressed condition, two factors served to ensure this lack of awareness. Firstly, presentation of the moving pattern was made to coincide with the phases of non-dominance of the field upon which it was superimposed. Secondly, the 'energy level' of the stimulus was reduced sufficiently for it to qualify as subliminal. This qualification was justified with the satisfaction of two criteria (cf. Dixon, 1971). Firstly, the stimulus was presented below the predetermined awareness threshold, where this threshold was defined as the lowest level of stimulus energy at which the subject ever reported seeing anything of the stimulus during the whole of the threshold determination procedure. Secondly, subjects reported retrospectively that they saw nothing of the stimulus, in either the suppressed or dominant conditions.

(*b*) The discrimination of movement is possible for a stimulus that is, in the context of binocular rivalry, currently non-dominant. Not only was the temporal course of rivalry found to be sensitive to the presence of a moving pattern within the currently non-dominant field, but the nature of the effects confirmed Levelt's thesis under conditions that favour its literal interpretation. Generalizing from this, it is proposed that *any* features that contribute to the strength of a stimulus, thereby influencing the course of rivalry in accordance with Levelt's thesis, are discriminated by the visual system whilst they are non-dominant, or phenomenally 'suppressed'.

(*c*) The discrimination of movement in a non-dominant field is possible even for a stimulus that is otherwise subliminal. The attenuation of non-attended material, if it occurs (cf. Treisman, 1964), must therefore be relatively unsubstantial.

(*d*) Finally, in confirming the literal interpretation of Levelt's thesis, the results suggest that the non-dominant stimulus is not only capable of influencing the perceptual alternation process (shift in attention) that characterizes rivalry, but actually takes precedence in this.

Bearing the analogy with dichotic listening in mind, these results suggest that a theory in line with the Deutschs' treatment of selective attention is the more applicable to rivalry. Sherrington can be said to have anticipated this in concluding from his studies on binocular flicker that 'only after the sensations initiated from right and left "corresponding points" have been elaborated, and have reached a dignity and definiteness well amenable to introspection, does interference between the reactions of the two eye-systems occur' (Sherrington, 1947, p. 379). Consistent with this Ramachandran *et al.* (1973*a*, *b*) have demonstrated that stereopsis can arise from a disparity between relatively abstract features of a pair of patterns (for example, linearity, texture density and contours formed by changes in grating direction). They argue from this that the discrimination of these features must precede the interactive process that determines whether the monocular images exhibit rivalry, or fuse to yield stereopsis. To the same effect, Somekh & Wilding (1973) have shown that whilst a word that is briefly presented to one eye may be masked by a brighter stimulus that is simultaneously presented to the contralateral eye, its meaning may nevertheless influence a subject's associations to the masking stimulus.

These findings, then, are consistent with recent data from the auditory domain which show that a relatively sophisticated analysis is undertaken on information residing in an unattended channel (cf. Lewis, 1970; Corteen & Wood, 1972).

Finally, consideration of some physiological data affords a little insight into the nature of the processes contributing to the present observations.

(*a*) With reference to the first, at least one study suggests a mediating role for midbrain structures. Thus Poppel *et al.* (1973) observed an increase in the amplitude of a patient's saccadic eye movements that was correlated with the eccentricity of a moving target presented within the 'cortically' blind area of his visual field. Because of the presence of the cortical lesion the patient remained totally unaware of the moving stimulus.

(*b*) Bearing in mind the parallel that has been drawn between the perceptual suppression occurring in binocular rivalry, and that occurring in amblyopia, a recently proposed theory of the deficit associated with strabismus (Ikeda & Wright, 1974) implicates the midbrain in the second observation. Ikeda & Wright propose that whilst the principal pathway from the amblyopic (suppressed) eye to striate cortex, via lateral geniculate nucleus, is non-functional, the parallel retino–superior colliculus pathway is able to function normally. Their own and other research has implicated the latter pathway in the discrimination of movement (e.g. Ikeda & Wright, 1972; Humphrey, 1968; McIlwain & Buser, 1967).

(*c*) With respect to the third finding, it is the case, as Ikeda & Wright demonstrate, that cells comprising the pathway to the superior colliculus are particularly sensitive to moving stimuli. So that, compared with cells comprising the principal pathway terminating in striate cortex, they are the more capable of responding to a moving stimulus of low contrast (Ikeda & Wright, 1972).

(*d*) Finally, the involvement of the superior colliculus in these studies is consistent with the fourth and rather paradoxical finding which suggests a dual nature to the visual system. Thus, whilst the one aspect is to be most intimately associated with the currently dominant (attended) stimulus, the other is to be more associated with the non-dominant (unattended) stimulus and with the rivalry alternations. The relatively independent functioning of the 'retino–geniculo–striate cortex' and 'retino–superior colliculus–temporal cortex' pathways (Gross, 1972; Killackey *et al.*, 1972; Snyder & Diamond, 1968) fulfils these requirements. Whilst the former has been assigned a mediating role in the focusing or sustaining of attention (Killackey *et al.*, 1972), the latter has been implicated in the assessment of the significance of a stimulus and in redirecting or shifting of attention (Butter & Hirtzel, 1970; Gerbrandt *et al.*, 1970; Gross, 1972; Pribram, 1971; Wilson *et al.*, 1972). Thus, for example, Gerbrandt *et al.* demonstrated that stimulation of inferotemporal cortex induces a state within striate cortex that is comparable to that which exists whilst the animal (monkey) is visually attentive. Whilst Wurtz & Goldberg (1972) have shown that the superior colliculus is involved in facilitating the eye movements that accompany shifts in attention.

To summarize, the physiological data relating to the notion of 'two visual systems' and their complementary roles (cf. Schneider, 1969) offer some insight into the processes underlying binocular rivalry and, more generally, selective attention. Extrapolating further, it may be proposed that the midbrain–temporal cortex aspect is instrumental in guiding a matching process in its sampling of the sensory data provided by the geniculo-striate system. It has been argued that it is just such a process that underlies perceptual (phenomenal) experience (MacKay, 1967).

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REFERENCES

BROADBENT, D. E. (1958). *Perception and Communication.* London: Pergamon Press.

BUTTER, C. M. & HIRTZEL, M. (1970). Impairment in sampling visual stimuli in monkeys with inferotemporal lesions. *Physiol. Behav.* **5**, 369–370.

CORNSWEET, T. N. (1962). The staircase method in psychophysics. *Am. J. Psychol.* **75**, 485–491.

CORTEEN, R. S. & WOOD, B. (1972). Autonomic responses to shock-associated words in an unattended channel. *J. exp. Psychol.* **94**, 308–313.

DEUTSCH, J. A. & DEUTSCH, D. (1963). Attention: some theoretical considerations. *Psychol. Rev.* **70**, 80–90.

DIXON, N. F. (1971). *Subliminal Perception.* London: McGraw-Hill.

GERBRANDT, L. K., SPINELLI, D. N. & PRIBRAM, K. H. (1970). The interaction of visual attention and temporal cortex stimulation on electrical activity evoked in the striate cortex. *Electroenceph. clin. Neurophysiol.* **29**, 146–155

GROSS, C. G. (1972). Visual functions of inferotemporal cortex. In R. Jung (ed.), *Handbook of Sensory Physiology* vol. VII/3: *Central Processing of Visual Information,* Part B. New York: Springer-Verlag.

HUMPHREY, N. K. (1968). Responses to visual stimuli of units in the superior colliculus of rats and monkeys. *Exp. Neurol.* **20**, 312–340.

Ikeda, H. & Wright, M. J. (1972*a*). Receptive field organization of 'sustained' and 'transient' retinal ganglion cells which subserve different functional roles. *J. Physiol.* **227**, 769–800.

Ikeda, H. & Wright, M. J. (1972*b*). Differential effects of refractive errors and receptive field organization of central and peripheral ganglion cells. *Vision Res.* **12**, 1465–1476.

Ikeda, H. & Wright, M. J. (1974). Is amblyopia due to inappropriate stimulation of the 'sustained' pathway during development? *Br. J. Ophthal.* **58**, 165–175.

Killackey, H., Wilson, M. & Diamond, I. T. (1972). Further studies of the striate and extra-striate visual cortex in the tree shrew. *J. comp. physiol. Psychol.* **81**, 45–63.

Levelt, W. J. M. (1966). The alternation process in binocular rivalry. *Br. J. Psychol.* **57**, 225–238.

Lewis, J. L. (1970). Semantic processing of unattended messages using dichotic listening. *J. exp. Psychol.* **85**, 225–228.

MacKay, D. M. (1967). Ways of looking at perception. In W. Wathen-Dunn (ed.), *Models for the Perception of Speech and Visual Form.* Boston: M.I.T. Press.

McIlwain, J. T. & Buser, P. (1967). Receptive fields of single cells in the cat's superior colliculus. *Exp. Brain Res.* **5**, 314–325.

Poppel, E., Held, R. & Frost, D. (1973). Residual visual function after brain wounds involving the central visual pathways in man. *Nature, Lond.* **243**, 295–296.

Pribram, K. H. (1971). *Languages of the Brain.* Englewood Cliffs, N.J.: Prentice-Hall.

Ramachandran, V. S., Rao, V. M., Sriram, S. & Vidyasagar, T. R. (1973*a*). The role of colour perception and 'pattern' recognition in stereopsis. *Vision Res.* **13**, 505–509.

Ramachandran, V. S., Rao, V. M. & Vidyasagar, T. R. (1973*b*). The role of contours in stereopsis. *Nature, Lond.* **242**, 412–414.

Schneider, G. E. (1969). Two visual systems. *Science, N.Y.* **163**, 895–902.

Sherrington, C. (1947). *The Integrative Action of the Nervous System.* London: Cambridge University Press.

Snyder, M. & Diamond, I. T. (1968). The organization and function of the visual cortex in the tree shrew. *Brain, Behav. Evol.* **1**, 244–288.

Somekh, D. E. & Wilding, J. M. (1973). Perception without awareness in a dichoptic viewing situation. *Br. J. Psychol.* **64**, 339–349.

Treisman, A. M. (1964). Verbal cues, language and meaning in selective attention. *Am. J. Psychol.* **77**, 206–219.

Wilson, M., Kaufman, H. M., Zieler, R. E. & Lieb, J. P. (1972). Visual identification and memory in monkeys with circumscribed inferotemporal lesions. *J. comp. physiol. Psychol.* **78**, 173–183.

Wurtz, R. H. & Goldberg, M. E. (1972). The primate superior colliculus and the shift of visual attention. *Invest. Ophthal.* **11**, 441–450.

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EXPERIMENTER BIAS AND SUBLIMINAL PERCEPTION

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It has been suggested that subliminal perception phenomena may be in part due to experimenter bias effects. Two studies that obtained positive evidence of subliminal perception were therefore replicated with experimenters tested under blind and not blind conditions. There was only marginal support for the subliminal perception hypothesis and, although there were fairly clear indications of diffuse experimenter effects, the evidence for the experimenter bias explanation of subliminal perception was not strong. The need for more extensive replications of subliminal perception researches is noted. It is argued that the experimenter bias hypothesis lacks detail and generality; it is essential for it to be examined in the context of theoretically substantial issues.

It was suggested by Neisser (1967) that subliminal perception effects may be mediated by 'demand characteristics'. This would be a serious assertion if it were more specific, and if it had an empirical basis. These studies examined a strong version of Neisser's hypothesis: that subliminal perception effects are mediated by experimenter bias (Rosenthal, 1966, 1969). A direct approach was adopted, replicating 'successful' paradigms with and without an experimenter-blind condition.

Two studies were selected which Neisser (1967) had mentioned in some detail, and which otherwise seemed quite well-known and were associated with strong and explicit theoretical predictions. The studies chosen for replication were those of Smith *et al.* (1959) and Spence & Holland (1962). Both were implemented with and without an experimenter-blind condition, and so it was possible to test both the subliminal perception hypothesis and the experimenter-bias hypothesis.

The most extreme form of the present experimenter-bias hypothesis states that there will be no subliminal perception effect except when, in some relevant sense, the experimenter is not blind (i.e. he can bias). According to the subliminal perception hypothesis the subliminal perception effects will occur both when the experimenter is blind and when he is not blind. There is of course room for a compromise hypothesis maintaining that the subliminal perception effects occurring when the experimenter is blind will be enhanced when he is not blind.

EXPERIMENT I

Spence & Holland (1962), arguing within a psychoanalytic framework, hypothesized that reduced awareness would lead to a diffusion of associative activity. In particular, they reasoned that a subliminal stimulus would be registered and prime an associative network in which it would produce more widespread and diverse activity than if it were presented supraliminally. Hence their experiment: the subject was presented with the word CHEESE either subliminally, supraliminally or not at all.

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He was then given a free-recall trial of a word list comprising associates and non-associates of the word CHEESE. Recall of CHEESE associates was facilitated only in the subliminal condition.

The present study involved a replication of this experiment using an individual test procedure in preference to the group testing of the original. Each assistant experimenter (E_A) administered the experiment under blind and not blind conditions.

Method

The experiment extended over two days for each E_A, who tested six subjects under each of the three cue conditions used by Spence & Holland (1962): subliminal, blank and supraliminal. Half of the subjects in each group were tested with E_A blind with respect to the cue condition. There was no simple way of preventing E_A from knowing when the subject was presented with a supraliminal cue, so the experimenter-blind manipulation strictly refers to the subliminal–blank distinction.

Individual word recognition thresholds were estimated prior to the cue treatment. Following the cue treatment, the Spence & Holland CHEESE word list was presented and the subject gave immediate written recall of this material.

Experimenters and subjects

The E_As were two male and two female undergraduate psychology students, familiar with subliminal perception evidence and expressing a belief in the validity of the phenomena. Each E_A found and tested 18 subjects aged between 18 and 60. In all there were 72 subjects (42 male, 30 female; mean age 27 years).

Procedure

Experimenter procedure. The initial briefing of the E_As emphasized that subliminal perception effects had been demonstrated effectively enough, but that it had been alleged that experimenter effects might overlay the findings. Therefore it was the objective of the present study to replicate the original finding before conducting further experiments based on this paradigm. The E_A was then advised that the CHEESE recall effect had been demonstrated clearly and beyond doubt and the effect was described in some detail to be sure that it was quite familiar. It was expected, wrongly as it turned out, that the E_As would know about Neisser's (1967) demand characteristics argument about subliminal perception. It was in any event feasible that E_As would infer the experimenters' concern about experimenter effects from the design of this study, and it was for these reasons that there was no attempt in Expt. I to conceal this concern. Nevertheless, the briefing was slanted to give E_A the impression that the primary objective was to replicate the subliminal perception effect and that experimenter effects were merely an inevitable nuisance factor. This should not have eliminated positive bias effects except of a deliberate kind.

The experimenter, entirely concealed from E_A and the subject by a large screen, was responsible for the operation of the tachistoscope. The subjects did not know that the experimenter was behind the screen. The recall protocols were sealed in envelopes by E_A as soon after recall as possible. No data were inspected until the experiment was completed and both experimenters scored all the data independently.

Preliminary threshold estimation. The recognition threshold for each subject was obtained for each of six words equated with CHEESE for length and word frequency. The words were typed in capitals on grey cards and were presented in a Cambridge Model tachistoscope with a plain white card in the inspection field. The simple ascending method of limits was used. The critical exposure duration was taken as four duration levels (generally 40 msec.) below the lowest exposure time obtained during this preliminary session.

Cue treatment. In the subliminal and blank conditions the respective cues were the word CHEESE or the symbols xxxxxx. The cue was presented tachistoscopically five times at 5 sec. intervals at the critical exposure duration determined in the preliminary session. Multiple presentations of the cue were used to reduce the chance that the subject inadvertently failed to receive the cue (e.g. because he blinked coincidentally with the presentation). Although repeated tachistoscopic

Table 1. *Analysis of variance of recall data: Expt. I*

Source	D.F.	M.S.	F
Between subjects	71		
Bias (A)	1	6·25	1·49
Cue (B)	2	0·88	0·27
Experimenters (E)	3	2·43	0·51
A × B	2	2·15	1·13
A × E	3	4·19	0·88
B × E	6	3·28	0·69
A × B × E	6	1·90	0·40
Subjects within groups	48	4·74	
Within subjects	72		
Recall category (C)	1	53·78	28·75*
A × C	1	0·25	0·08
B × C	2	5·34	3·66
E × C	3	1·87	0·69
A × B × C	2	1·27	0·33
A × E × C	3	3·16	1·16
B × E × C	6	1·46	0·54
A × B × E × C	6	3·82	1·40
C × subjects within groups	48	2·73	

* $P < 0{\cdot}05$.

presentations increase the probability of word recognition (Haber & Hershenson, 1965) it is unlikely that the effect is important for a duration as far below threshold as those used here (Haslerud, 1964). In the supraliminal condition CHEESE was presented four times at 5 sec. intervals at an exposure duration of 1 sec. In all cases the subject was instructed to say if possible what was being presented.

Recall procedure. Following immediately after the cue treatment, the subject was presented with a deck of 20 plain playing cards on which were typed in capitals the words from the Spence & Holland list (excepting their primacy and recency buffer items since a fixed order was not used in this replication). These words were: (*a*) CHEESE associated: COW, BREAD, CAVE, MOON, COTTAGE, GREEN, BRICK, SMELL, MOUSE, SOUR; (*b*) control words: FLAG, ARCH, CHAIR, REST, GIFT, FRAIL, SAND, HEDGE, MINOR, TRUNK. The order of the recall list was produced by E_A by shuffling the deck thoroughly for each subject. The presentation of the list was controlled by the subject but was, following a demonstration by E_A, intended to be at about one word per second.

Results

In the source study Spence & Holland used differential recall of CHEESE associates as the dependent variable. This was defined as the difference between the number of CHEESE associates and the number of control words recalled. Several versions of the recall scores were examined in the present study and the conclusions from them were similar in all essential respects. The analysis of variance summarized in Table 1 is typical of the pattern of statistical significance (the same in all analyses) and of the magnitude of the effects. This pattern also emerged if the supraliminal condition (for which the blind treatment was suspect) was excluded.

The only significant effect on recall was that corresponding to the difference between CHEESE associates and control words ($F = 28{\cdot}75$; d.f. $= 1, 3$; $P < 0{\cdot}05$). This can be seen in Table 2, which presents mean recall scores averaged across E_As.

An overall parametric analysis of variance was preferred to the piecemeal nonparametric treatment that otherwise would be necessary to examine the data in all

Table 2. *Mean recall averaged across assistant experimenters: Expt. I*

		Recall category	
Bias condition	Cue	Cheese associates	Control words
Blind	Subliminal	5·67	3·50
	Blank	4·67	3·67
	Supraliminal	4·17	3·92
Not blind	Subliminal	5·42	3·83
	Blank	5·25	3·75
	Supraliminal	5·33	4·50

the relevant aspects. It was of course essential that this analysis should allow the same basic question to be put as in the source study.

The main finding of the source study was not replicated, since the cue × recall category (representing Spence & Holland's differential recall variable) was not significant at the 5 per cent level ($F = 3{\cdot}56$; d.f. = 2, 6; $0{\cdot}10 > P > 0{\cdot}05$), although it just attained significance at the 10 per cent level. Entirely crucial to the experimenter bias hypothesis is the cue × recall category × bias interaction. This also was not significant ($F = 0{\cdot}33$; d.f. = 2, 6; $P > 0{\cdot}05$), nor was any other interaction which might qualify the effect of these variables. There were no significant interactions with the experimenter factor.

The analysis of variance summarized in Table 1 was constructed on the assumption that experimenters should be treated as a random factor (using interactions with experimenters as error terms for fixed main and interaction effects, but using interactions with subjects otherwise). The intention was to aim for generality in this way, but it makes no difference to the above conclusions if experimenters are (improperly) treated as a fixed factor. Nor is anything gained by the dubious device of pooling the interactions involving experimenters to form a common error term; for instance, the cue × recall category interaction becomes statistically even less remarkable ($F = 1{\cdot}98$; d.f. = 2, 55; $P > 0{\cdot}10$). A planned comparison (Kirk, 1968) of the means of the subliminal condition against the combined means of the supraliminal and blank conditions approached significance ($F = 5{\cdot}31$; d.f. = 1, 6; $0{\cdot}1 > P > 0{\cdot}05$), suggesting a marginal tendency for the recall of CHEESE associates to be additionally facilitated when the cue word was presented subliminally.

An experimenter effect was found in the preliminary threshold data. An analysis of variance of the logarithms of the critical exposure durations recorded for each subject indicated that the experimenter factor was significant ($F = 4{\cdot}864$; d.f. = 3, 48; $P < 0{\cdot}01$) and the experimenters × bias × cue interaction was significant ($F = 2{\cdot}371$; d.f. = 6, 48; $P < 0{\cdot}05$). In this last case there was no discernible pattern, although there was little variability in thresholds when E_A was blind. This is reassuring since it suggests that E_As' initial treatment of their subjects was, as intended, independent of the cue condition; this is consistent with the notion that E_As did not receive any useful information from the concealed experimenter when the blind condition applied. The overall mean preliminary threshold was 135 msec.

EXPERIMENT II

In an attempt to exaggerate any effect there might be due to the expectations of the experimenter, one of the E_As from Expt. I tested further subjects, ostensibly under the subliminal and blank cue conditions. In fact, half of the subjects were tested, unknown to the E_A, with the opposite cue treatment to that which she expected. In this way, it was possible to examine the effects of the cue and of the E_A's expectancy in combination and in opposition.

Table 3. *Mean recall scores and the expectations of the assistant experimenter: Expt. II*

		Recall category	
Expected cue	Actual cue	Cheese associates	Control words
Subliminal	Subliminal	5·10	3·90
	Blank	4·30	3·70
Blank	Subliminal	4·20	3·50
	Blank	5·30	3·20

Method

Experimenter and subjects

One of the female E_As from Expt. I was employed to contact and run 40 further subjects in a subset of the conditions of the first study. The E_A was not asked to record details of subjects but to ensure that they were in the same age range as before (18–60 years).

Procedure

Experimenter procedure. The E_A was told that in Expt. I her results in the subliminal *v.* blank condition were the only ones to attain statistical significance and that it was now intended to investigate the reliability of the effects. She was asked to test 20 subjects under the subliminal CHEESE cue condition and another 20 subjects under the blank cue condition. Correct information was given about the cue for the first 10 subjects on each of the two extra experimental sessions required, but the cue was opposite to what E_A had been told for the second set of 10 subjects in each session. The E_A was debriefed at the end of data collection.

Preliminary threshold estimation and post-recall check. The preliminaries for each subject were the same as in Expt. I. After the recall test the subject was asked to indicate on a seven-point scale the degree of certainty with which he judged the pre-recall subliminal cue to be CHEESE. This was intended to provide an indirect check on whether, in the event that E_A was able to bias, the subject was able to detect this bias. It incidentally acted as a secondary check on the subliminality of the cue. Most subjects (34/40) showed uncertainty about the cue (rating 4) and the distribution of scores was so even that the data were not analysed further.

Cue treatment and recall procedure. The pre-recall treatment for a given subject consisted of either the subliminal or blank condition of Expt. I. The recall procedure was unchanged from Expt. I.

Results

Conclusions for the various recall indices (absolute or relative scores) were the same and absolute recall scores are reported here (means are presented in Table 3).

As in Expt. I only recall category contributed significantly to the total variance; again recall of CHEESE associates was reliably better than of control words. If the E_As were not biasing and there was a subliminal perception effect alone, then the cue ×

recall category interaction would be significant; this was not so ($F = 0{\cdot}35$; d.f. $= 1, 36$; $P > 0{\cdot}05$). On the other hand, if there was no subliminal perception effect but E_A could bias, then the expectancy × recall category interaction would be significant; this did not occur ($F = 0{\cdot}54$; d.f. $= 1, 36$; $P > 0{\cdot}05$). A joint hypothesis that there would be both subliminal perception and experimenter bias effects was not supported; the expectancy × cue × recall category interaction was not significant $2{\cdot}16$; d.f. $= (F = 1, 36$; $P > 0{\cdot}05)$.

Table 4. *Comparison of source and replication studies: Expt. I*

Basis of comparison	Source study (Spence & Holland, 1962)	Replication study
Subjects	$n = 59$	$n = 72$
Source of subjects	Undergraduate classes	Selected by E_A locally in university precinct
Procedure	Group-testing; and subliminal and blank groups run together	Individual
Display	Slide projector + shutter	Tachistoscope
Exposure duration	$\frac{1}{160}$ sec. and a post-experimental check	Individually determined. Range 30–100 msec.
Blank cue condition	Subjects looked away from screen	Subliminal presentation of XXXXXX
Recall list	Word list of 10 CHEESE associates + 10 control words + three primacy and three recency 'buffer' items	Word list of 10 CHEESE associates + 10 control words (same as source)
Presentation order of recall list	Fixed	Random
Presentation modality of recall list	Auditory (by experimenter)	Visual (by subject)
Experimenters	'Naive instructor'	Four assistant experimenters + experimenter behind screen to operate display

Preliminary discussion of Experiments I and II

Procedural differences between the source study and the experiments reported above may account for the failure to replicate. On balance, the advantage in terms of sensitivity to subliminal perception effects lies with the replication study. Thus the use of an individual-testing procedure was an improvement over the original group-testing approach as exposure durations could be used that were more likely to be appropriate for a given individual, and it was easier to monitor the subject's attention to the task. Other differences (see Table 4) seem to be the kinds of variation to which the phenomena would be insensitive. The procedural differences therefore favoured the replication study as a detector of subliminal perception effects.

The failure to replicate cannot readily be attributed to experimenter bias in the original study since, although diffuse effects werefound on the preliminary thresholds, the blind *v.* not blind manipulation had no effect on the main dependent variables in the present study. Concerning the recall task the overall level of performance was on the whole better than in the original study, but this was probably due to the difference in effective list length; in the source study the primacy–recency buffer items which were omitted in the replication were not included in the recall score.

An alternative explanation for the failure to replicate is that there was a negative experimenter effect (Dixon, 1971) in the present study; in some way the subjects were made unresponsive to the subliminal stimuli. This is not a convincing explanation because *inter alia* it runs counter to the expectations of both experimenters and E_As, but it is difficult to rule it out in this or any other study failing to find a subliminal effect.

In terms of statistical inference, either Spence & Holland committed a Type I error or a Type II error has been committed here. Again the balance seems to favour the replication study which used more powerful statistical tests, but comparison of power is not straightforward because the degrees of freedom on which the hypotheses were tested here were small (because the design was hierarchical). For this reason individual analysis, for each E_A considered separately, might be preferred, since the degrees of freedom would be more comparable to the original analysis. When this was done there was still no significant effect (other than recall category) for any E_A despite the increased degrees of freedom. Furthermore, as has been seen above, the use of pooled error terms did not lead to a different conclusion.

The other way in which the source and replication studies differ is in relation to the generality of the effect of recall category. In both replication experiments there was a clear superiority in recall of the CHEESE associates over the control words. In Spence & Holland's experiment this advantage was not statistically reliable in either of their blank and supraliminal conditions. The present findings of a general advantage to the associated words are in line with evidence on the relation between free recall and intralist associations (Deese, 1959; Rothkopf & Coke, 1961). In the light of this evidence it could be argued that Spence & Holland's findings were mediated not by the facilitative effect of the subliminal cue but by a depressive influence acting in the other conditions.

EXPERIMENT III

The second source study was the experiment by Smith *et al.* (1959). This experiment investigated the effects of the subliminal presentation of a word (HAPPY or ANGRY) alternating with a clearly visible line drawing of an expressionless face. The principal finding was that verbal descriptions of the face were judged subsequently to be significantly different on a dimension related to the subliminal cue. Apart from the inclusion of an experimenter blind *v.* not blind comparison, the major difference between the source experiment and the replication concerned the method of response recording, since in the replication the subject used a rating scale.

Method

Each subject rated a series of pictures of expressionless faces on a 'happy–angry' scale. Each picture was periodically interrupted by the subliminal presentation of one of the words HAPPY or ANGRY. The E_A either knew (not blind) or did not know (blind) which word was being used as the subliminal cue.

Stimulus material

Eight line drawings of faces rated as neutral by independent judges on a five-point HAPPY–ANGRY scale were used in the replication.

Experimenters and subjects

The E_As were one male and six female undergraduate psychology students who had expressed a conviction about the validity of the evidence on subliminal perception. Each E_A tested 24 subjects aged between 18 and 60. In consequence, 168 subjects (108 males, 57 females; unrecorded data for three subjects) participated in the experiment.

Procedure

Experimenter procedure. The E_As were advised of the general findings of the source experiment and it was stated that they would be replicating other workers' findings if they obtained subliminal perception effects. They were unaware of the experimenters' interest in experimenter effects. A more elaborate briefing was judged to be unnecessary because the E_As in Expt. I were found to be naive with respect to the demand characteristics hypothesis of subliminal perception. Each E_A attended for two sessions and the first was run with E_A not blind with respect to the subliminal cue word. At the end of this session E_A was instructed that in the second session he would not know what was the subliminal cue, so that a comparison of his performance, blind and not blind, could be made.

On the first session each E_A tested six subjects with HAPPY as the subliminal cue and six subjects with ANGRY as the cue. These were run in two blocks with the same cue within a block, and the order of blocks was alternated between E_As. Thus it was ensured that E_A would have a settled notion of what cue was being used. In the second session the cue word, which was taped into position by the experimenter, was randomly changed between subjects by the experimenter in the interval when E_A was fetching the next subject. Six subjects were tested with HAPPY as the cue, and six with ANGRY. The experimenter was in a nearby room and did not meet or communicate with E_A during the running of the experiment.

Pre- and post-experimental threshold procedure. Four words matched for length and word frequency with HAPPY and ANGRY were used in conjunction with an ancillary Cambridge Model tachistoscope to obtain the critical duration for the cue word. The ascending method of limits was used and the subliminal exposure duration was then taken as that level which was four duration steps below the lowest previously obtained threshold (averaging 87 msec.).

The threshold for each subject was again established at the end of the main task. If the post-experimental threshold was lower than or equal to the pre-experimental threshold, the subject was replaced; this was necessary for six subjects, evenly distributed among E_As. The words were typed in capitals on grey cards and the inspection field was a blank white card.

Cue treatment and mood rating task. The subliminal cue word was inserted by E_A in the not blind session, but was taped in position by the experimenter in the blind session. The cards bearing the line-drawings of faces were inserted (one at a time) in the inspection field of the tachistoscope, and on a given trial the subject was given 10–15 sec. to inspect the drawing. During this period E_A triggered a repeat circuit which presented the cue 10 times at the chosen critical duration at intervals of 0·5 sec.

The subject was asked to judge whether each face looked happy or angry, and he indicated his judgement on a card marked with a seven-point rating scale. There was a separate card for each face. The scale ranged from 1 for 'extremely happy', through 4 for 'neither happy nor angry', to 7 for 'extremely angry'. The order of presentation of the eight faces was separately randomized for each subject.

Results

The eight ratings for each subject were combined into two sets of four, corresponding to the first and second phases of the experiment, which were in fact undifferentiated as far as the subject was concerned. These pooled ratings were subjected to an

Table 5. *Analysis of variance of 'happy-angry' ratings: Expt. III*

Source	D.F.	M.S.	F
Between subjects	167		
Bias (A)	1	0·1163	0·04
Cue (B)	1	0·3127	2·37
Experimenters (E)	6	0·2767	1·04
A × B	1	0·0984	0·21
A × E	6	0·3181	1·21
B × E	6	0·1317	0·50
A × B × E	6	0·4626	1·75
Subjects within groups	140	0·2649	
Within subjects	168		
Phase (C)	1	0·0017	0·00
A × C	1	0·0672	0·11
B × C	1	0·1163	0·27
E × C	6	0·4787	1·42
A × B × C	1	0·6475	1·83
A × E × C	6	0·6310	1·87
B × E × C	6	0·4240	1·26
A × B × E × C	6	0·3528	1·05
C × subjects within groups	140	0·3366	

Table 6. *Mean ratings, averaged over assistant experimenters, on the 'happy–angry' scale: Expt. III*

		Cue condition	
Bias condition	Phase	Happy	Angry
Blind	First	4·27	4·25
	Second	4·26	4·33
Not blind	First	4·14	4·36
	Second	4·24	4·21

Table 7. *Mean ratings for male and female subjects classified by cue and bias conditions*

(Numbers of subjects per cell are shown in parentheses. $\mathrm{MS}_{\mathrm{error}}$ = 0·126 with 157 d.f.)

		Cue	
Sex of subject	Bias condition	Happy	Angry
Female	Blind	4·11 (14)	4·31 (10)
	Not blind	4·35 (18)	4·19 (15)
Male	Blind	4·33 (27)	4·31 (30)
	Not blind	4·07 (24)	4·34 (27)

analysis of variance, summarized in Table 5. There were no significant main or interaction effects. This applied regardless of whether the experimenter factor was treated as random or fixed, and whether error terms were pooled or not. In particular, there was no subliminal perception effect (cue), nor an experimenter-bias effect (bias), nor an interaction between them. Furthermore, neither of these effects showed any sign of developing within a session (no interaction with phase).

The general impression of numerical calm can be seen in Table 6, which shows the mean ratings averaged over E_{A}s.

Table 8. *Comparison of source and replication studies: Expt. III*

Basis of comparison	Source study (Smith *et al.*, 1959)	Replication study
Subjects	$n = 20$	$n = 168$
Source of subjects	VA hospital	Selected by E_A locally in university precinct
Procedure	Individual testing	Individual testing
Display	Tachistoscope	Tachistoscope
Exposure duration	Individually determined. Increased from 4 to 20 msec. (modal value) across 47 exposures	Individually determined + post-experimental check. Fixed duration throughout
Supraliminal material	Line-drawing of face	Line-drawings of eight faces
Response indicators	Verbal report of 'mood changes', subsequently rated by two judges. Report latency. Imagery reports	'Mood' ratings on seven-point HAPPY–ANGRY scale
Design	HAPPY *v.* ANGRY: within-subjects variable. Same sequence of HAPPY and ANGRY for all subjects	HAPPY *v.* ANGRY: between-subjects variable
Experimenters	E_1 to control display. E_2, ignorant of stimulus, to prompt subjects' responses.	Seven assistant experimenters

Analysis of variance of the pre-experimental log thresholds indicated a small but significant main effect of experimenters ($F = 2{\cdot}40$; d.f. = 6, 140; $P < 0{\cdot}05$). There was no other significant main or interaction effect, although thresholds tended to be higher when E_As were not blind than when blind with respect to the cue ($F = 4{\cdot}36$; d.f. = 1, 6; $0{\cdot}10 > P > 0{\cdot}05$); this was true for six of the seven E_As. The reliability of this effect is emphasized by an analysis of variance of the post-experimental log thresholds; the bias factor was now significant ($F = 8{\cdot}74$; d.f. = 1, 6; $P < 0{\cdot}05$), and by this stage all seven E_As were recording higher thresholds in the not blind condition. However, the main effect due to experimenters was no longer significant in the post-experimental threshold session ($F = 0{\cdot}92$; d.f. = 6, 140; $P > 0{\cdot}05$).

Because almost twice as many males as females were selected by the E_As, an analysis of the ratings with sex as a factor had to sacrifice one of the independent variables. A subsidiary analysis of variance was computed ignoring the experimenter factor but retaining the cue and bias factors. This unequal-cell analysis of variance, by the unweighted means method, was based on the mean rating per subject, and revealed a significant sex × cue × bias interaction ($F = 7{\cdot}79$; d.f. = 1, 157; $P < 0{\cdot}01$). *Post hoc* comparisons (Scheffé, 1959) between the happy and angry conditions revealed a significant difference for male subjects tested by not blind E_As ($F = 7{\cdot}36$; d.f. = 1, 157; $P < 0{\cdot}01$). This difference (see Table 7) is the only indication that E_As were biasing their subjects directionally.

Preliminary discussion of Experiment III

The main analysis contained no support for the subliminal perception hypothesis and in this sense Expt. III failed to replicate its source study. It is important again to consider whether procedural differences were sufficient to account for the failure to replicate.

Inspection of Table 8 suggests that there was little difference in terms of sensitivity to subliminal perception effects. The difference between response indicators is a major one but it is not clear where the advantage in sensitivity lies. The rating-scale procedure was preferred on several grounds: it seemed to emphasize to the subject and E_A what were the appropriate dimensions of judgement; it seemed that experimental error would be reduced by excluding judges as a source of variability; and it supplied a more immediate and direct means of quantifying response processes. On the other hand, it could be countered that the use of verbal reports in the original study was more suitable because it involves less restriction of the influence of the subliminal cue. Unfortunately there is no convincing independent empirical justification for the use of one method as opposed to the other.

Sensitivity to the experimental treatment can also be discussed in relation to the design status of the cue factor. In the original study, the subject was presented with both HAPPY and ANGRY as subliminal cues, while in the replication the subject was presented with either HAPPY or ANGRY but not both. All else being equal, a within-subjects comparison is generally more sensitive to a given treatment effect than a between-subjects comparison, and so this seems to indicate a sensitivity advantage to the source study. But it is not obvious that all else was equal since the dependent variable and the statistical methods that were applied (including of course the associated degrees of freedom) also differed. It is therefore not easy to decide which study would be at an advantage regarding sensitivity to subliminal perception effects, particularly since theoretical arguments can be made in both cases that obscure the question further.

The failure to replicate could be attributed to differences in the subject populations. It has been suggested (Dixon, 1971) that individuals in whom primary process thinking is more dominant may be more receptive to subliminal stimulation. However it is not clear that there is an advantage in this respect one way or the other except insofar as the source study used hospital patients most of whom seemed to have had psychiatric symptoms.

The defences against artifacts also differ between the two studies. The exposure duration in the source study was progressively increased towards the pre-experimental threshold level and in this way it would seem to have given some opportunity for the use of partial cues. In the replication study greater protection against the use of partial cues was achieved by eliminating subjects whose post-experimental threshold fell to the level of the critical duration used during the experiment.

Experimenter bias is another potential source of artifacts. The blind *v.* not blind manipulation of the replication study failed to produce an effect on the main dependent variable, which suggests that experimenter bias is not the critical difference between the two studies. On the other hand, when subject sex was taken into account, the replication study did yield evidence of experimenter bias; positive results were obtained with male subjects when the E_As were not blind and able to bias. This interaction effect was quite small and accounted for no more than 4 per cent of the total variance, and since the analysis was conducted *ex post facto*, the result has only a tenuous status.

Somekh & Wilding (1973) conducted a successful constructive replication of the source study of Expt. III, using a binocular rivalry paradigm to arrange that the subject was unaware of the cue. This was a technically impressive study which incorporated a device for 'hopefully eliminating any possibility of experimenter bias ...'; the 20 stimulus slides were numbered and presentation orders were subsequently decided by reference to random number tables. But this is not a method, however well intentioned, that prevents the experimenter from being informed about the stimuli. Furthermore, partial knowledge of the stimulus sequence could be enough to mediate bias. Indeed, this becomes increasingly plausible in Somekh & Wilding's study when it is realized that the stimuli, which were replicated, effectively formed only five distinct categories. Thus the incidental acquisition of the relevant associations was a real possibility. The basis for directional influence by the experimenter, irrespective of his intent, could reside in partial information about the critical stimulus categories (or category).

Although in the crucial subliminal session of Somekh & Wilding's study there was a significant difference in average mood ratings when the critical cues (HAPPY and SAD) were presented, the results in detail present a difficulty for the subliminal perception hypothesis which can be resolved by the partial information assumption. In Expt. I the SAD mean seems to have been homogeneous with the several control means but significantly different from the HAPPY mean. The situation in Expt. II changed somewhat, so that it was now the HAPPY mean which was similar to the control means but still significantly different from the SAD mean. It is conceivable that this pattern of results could have come about through the experimenter's inadvertent knowledge of one or two stimuli (HAPPY in Expt. I and SAD in Expt. II).

Smith *et al.* (1959) also tried to eliminate experimenter bias, with E_1 to operate the tachistoscope and E_2 to record and prompt the subject's responses. But since the presentation sequence was the same for all subjects and included blocks of HAPPY and ANGRY stimuli, it seems possible for E_2 to have acquired some knowledge of the stimulus order. The judged mood of the face varied according to the cue but the effect was more marked for HAPPY than for ANGRY. Thus the incomplete nature of the findings of both Smith *et al.* (1959) and Somekh & Wilding (1973) is consistent with the view that experimenter bias was not successfully ruled out and that it unwittingly became effective via the experimenter's partial knowledge of the stimulus sequence.

GENERAL DISCUSSION

Experimenter bias

From the results of these studies it is tempting to conclude that experimenter bias is not a sufficiently drastic or far-reaching phenomenon to be of further concern to subliminal perception theorists. This would be premature in view of the suggestion of positive bias in Expt. III, the discussion of Somekh & Wilding's (1973) study and Dixon's (1971) counter-claim about negative bias. What seems clear is that experimenter bias is, like subliminal perception, certainly a fragile phenomenon. Whether the experimenter bias effect is so specific in the case of subliminal perception as to be dependent on the subject's sex, as these results suggest, remains for a more searching study to determine. It also remains for further experiments to discover whether the experimenter's influence is stronger in less structured tasks, which may be more prone to experimenter bias effects (Masling, 1966; Barber & Silver, 1968).

The generality of experimenter bias has been questioned by several writers (Barber & Silver, 1968; Compton, 1970; Stewart, 1971). Discussions of the generality of the effects have emphasized the inconsistency of the phenomena, the unrepresentative nature of the experimenters employed, and the limited range of experimental tasks and situations that have been studied. In relation to the third criticism, it is striking that the work on experimenter effects rarely touches issues of any substance. There is much evidence of a psychologically and theoretically impoverished kind (e.g. person perception studies with no theoretical content). Studies like the present ones, where there is a general psychological hypothesis, are exceptions. In the light of the difficulties experienced here of pinpointing experimenter bias effects, one could speculate that bias may be more likely in circumstances where the experimenter has to make an 'effort after meaning'. Paradoxically therefore it may be the case that experimenters bias *less* where they do not have to infer or define for themselves the larger objectives of the study.

It is a simple matter for an investigator to attribute subliminal perception – or any other phenomenon he finds objectionable – to the operation of experimenter bias and demand characteristics. For this assertion to be taken seriously it is necessary to supply more than analogical evidence and this study has initiated this process for subliminal perception research. There are signs in the data presented above that suggest that experimenter bias may be effective in this context. The weakness of the effect is not reassuring, since the phenomena of subliminal perception are themselves reputedly small and difficult to detect. If further studies confirm these tentative conclusions it will be necessary to make explicit what the 'demand characteristics' are, how they manifest themselves, how they function, how and when experimenters bias their subjects. The present evidence that there were experimenter effects on threshold estimates indicates that experimenters can affect measures used in perception research. If the dependent variable in a study itself depends on the prior determination of a critical duration or intensity then it too could be subject to indirect but systematic influence.

Negative bias

In the light of the objectives of the present investigation it is difficult for us to enthuse about the possibility that the results were due to negative bias. Before discussing this issue it should be noted that there is a more compelling reason for being guarded about the use of negative bias as an explanatory concept. The difficulty arises because negative bias predicts no subliminal perception effect, an outcome which also follows if the subliminal perception hypothesis is not valid.

Moreover, a joint hypothesis consisting of the subliminal perception hypothesis (for positive instances of the effect) and negative bias (for null instances) seems *a priori* to be incontrovertible, and therefore unacceptable in such a comprehensive form. It is necessary not only to state ground-rules for obtaining subliminal perception effects, but also for their eradication. Our preference, in line with the rationale for these experiments, would be for negative bias to be manipulated and demonstrated experimentally. This is not to say that it would be unprofitable to seek for common features of studies which fail to demonstrate subliminal perception, since clearly this is likely to provide the major source of ideas as to what constitutes negative bias.

It will perhaps be appreciated that we have little to offer in the way of pointing to potential defects of the present experiments in relation to negative bias. It has been suggested (by a consulting editor) that the presence of the experimenter, although entirely concealed, in the room with E_A and the subject during Expts. I and II may have served to introduce negative bias, although this could not be a criticism of Expt. III. On the other hand, common to all three experiments was the fact that all E_As were relatively inexperienced. Their management of the experimental procedures may have served to obscure a subliminal perception effect, although it is arguable whether this is a case of negative bias. It is of course easy to use this line of reasoning to suggest that it is only with experience that an investigator can come to produce positive results in subliminal perception studies. However, if the effect was one of E_As injecting 'noise' into the data and hence swamping the effects of subliminal perception, it should follow that unaccounted for variability would be high. Direct comparison of error variances between source and replication studies was not possible because non-parametric analyses were used originally. The indirect and perhaps hazardous approach was therefore adopted of computing the mean squares for common main and interaction effects in Expt. I based on Spence & Holland's (1962) mean recall scores. The mean squares for the source study for cue, recall category and cue × recall category were found to be 2·98, 19·05 and 7·56 respectively (unweighted means solution), and in the replication the corresponding values were 0·88, 53·78 and 5·34 (Table 1). These variances seem quite comparable and, in particular, the crucial interaction variance was larger in the source study and would also have been declared significant had it occurred here. This suggests that the present error variances were not inflated and that E_As were not introducing spurious variability into the data on a gross unselective basis. If negative bias was active, then it was of a somewhat subtle and selective kind. This is a little puzzling, since it seems to be at odds with how it might be expected to be manifest according to Dixon's (1971) discussion of the problem. It appears that this cannot be easily resolved without appropriate research. This seems to require an independent investigation which incorporates an unequivocal demonstration not only of negative bias but also of subliminal perception.

Replicating subliminal perception effects

The procedural differences between the source and replication studies were intended to favour the latter in terms of sensitivity to subliminal perception,* and in any event it seems reasonable to describe the latter as constructive replications (Lykken, 1968). Failure to replicate the original findings does not, of course, constitute disproof of the subliminal perception hypothesis, although it reduces the generality of the data base on which the hypothesis rests. It seems at least to follow that subliminal perception effects are weak and can easily be made to disappear. This weakness of the phenomenon (Dixon, 1971) is in fact one of the strengths of the subliminal perception hypothesis, since it is easy to argue for the inadequacy of any study that fails to obtain positive evidence.

In terms of statistical significance the present findings did not confirm the source experiments. It could be argued that the numerical pattern of differential recall scores in Expt. I was the same as Spence & Holland's, and the ratings in Expt. III were on average in the expected direction. But this approach would have to be applied across the board, and it breaks down for Expt. II. Furthermore, to achieve significance in Expt. I, twice as many E_As would have to be employed, and Expt. III could be continued indefinitely without the cue effect becoming statistically significant. Even in these terms it is hard to construe the present data as support for the subliminal perception hypothesis.

The present experiments seem to cast doubt on the replicability of the original findings and not necessarily because of the possibility of experimenter bias. Dixon (1971) has called attention to the need to replicate his own experiments. The need is evidently more extensive if the well-publicized effects of the present source studies can be so easily dissipated as they were here. It should be emphasized that the original findings may be valid and that the present outcome could be due to negative bias. Otherwise the most parsimonious explanation of our failure to replicate is that the two source studies involved Type I errors and we were therefore chasing a statistical will-o'-the-wisp.

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* A reviewer has suggested that the source study for Expt. I was more sensitive to subliminal perception effects than the replication, since the exposure duration in the former was lower than in the latter. If subliminal perception effects are inversely related to exposure duration then this could explain why the source study obtained positive results and the replication did not. But because there were instrumentation differences as well as unmentioned factors like luminance, dark adaptation and so on, it would be hazardous to compare sensitivities on the basis of exposure duration alone. Moreover, it seems likely that the exposure duration used by Spence & Holland was quite close to threshold, since they found it advisable to discard some data because of doubts about the subliminality of the stimulus. This suggests that the exposure durations were effectively very similar.

REFERENCES

BARBER, T. X. & SILVER, M. J. (1968). Fact, fiction, and the experimenter bias effect. *Psychol. Bull.* **70** (6, pt 2), 1–29.

COMPTON, J. W. (1970). Experimenter bias: reaction time and types of expectancy information. *Percept. mot. Skills* **31**, 159–168.

DEESE, J. (1959). Influence of inter-item associative strength upon immediate free recall. *Psychol. Rep.* **5**, 305–312.

DIXON, N. F. (1971). *Subliminal Perception: the Nature of a Controversy.* London: McGraw-Hill.

HABER, R. N. & HERSHENSON, M. (1965). The effects of repeated brief exposures on the growth of a percept. *J. exp. Psychol.* **69**, 40–46.

HASLERUD, G. M. (1964). Perception of words as a function of delays between and summation of subliminal exposures. *Percept. mot. Skills* **19**, 130.

KIRK, R. E. (1968). *Experimental Design: Procedures for the Behavioural Sciences.* Belmont, Calif.: Brooks/Cole.

LYKKEN, D. T. (1968). Statistical significance in psychological research. *Psychol. Bull.* **70**, 151–159.

MASLING, J. (1966). Role-related behaviour of the subject and psychologist and its effects upon psychological data. In D. Levine (ed.), *Nebraska Symposium on Motivation.* Lincoln: University of Nebraska Press.

NEISSER, U. (1967). *Cognitive Psychology.* New York: Appleton-Century-Crofts.

ROSENTHAL, R. (1966). *Experimenter Effects in Behavioural Research.* New York: Appleton-Century-Crofts.

ROSENTHAL, R. (1969). Interpersonal expectations: effects of the experimenter's hypothesis. In R. Rosenthal & R. L. Rosnow (eds.), *Artifact in Behavioural Research.* New York: Academic Press.

ROTHKOPF, E. Z. & COKE, E. U. (1961). The prediction of free recall from word association measures. *J. exp. Psychol.* **62**, 433–438.

SCHEFFÉ, H. (1959). *The Analysis of Variance.* New York: Wiley.

SMITH, G. J. W., SPENCE, D. P. & KLEIN, G. S. (1959). Subliminal effects of verbal stimuli. *J. abnorm. soc. Psychol.* **59**, 167–177.

SOMEKH, D. E. & WILDING, J. M. (1973). Perception without awareness in a dichoptic viewing situation. *Br. J. Psychol.* **64**, 339–349.

SPENCE, D. P. & HOLLAND, B. (1962). The restricting effects of awareness: a paradox and an explanation. *J. abnorm. soc. Psychol.* **64**, 163–174.

STEWART, C. G. (1971). Consistency, generality, magnitude, and significance of experimenter expectancy effects in human research. *Psychol. Rec.* **21**, 449–458.

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CONTEXT, MEANING AND THE VALIDITY OF RESULTS IN PSYCHOLOGICAL RESEARCH

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This paper attempts to illuminate the problems of outcome validity in psychological inquiry by reference to the two fundamental characterizations of research results: (1) their context of applicability, and (2) their causal meaning. The failure of a research proposition to hold within its intended context of generalization is labelled as *context invalidity* and is characterized as one sense of a statistical interaction between the treatment and the background of a research. A second sense of such interaction is discussed in relation to the notion of *treatment invalidity*. Specifically, this sense is defined as the implication that an effect might be causally attributed to some composite of the treatment and background rather than to the treatment as such. The progression of causal explication is characterized as the successive establishment of conclusion, treatment and construct validities. Misclassifications of validity types, the twofold implication of the perils to 'external validity', the priority issue between 'internal' 'and external' validities, and misconstruals of the requirement of representative sampling are discussed in light of the context–meaning analysis above.

The problems of test validity have received intensive attention in the methodological literature (e.g. Cronbach & Meehl, 1955). However, no comparably thorough analysis has been accorded the issue of outcome validity within psychological research. Widely known is the distinction between internal and external validity (Campbell & Stanley, 1966). Internal validity was defined as the question whether the experimental treatment within the specific experiment did, in fact, make a difference. By contrast, external validity was defined as the problem of generalizability, i.e. 'to what populations, settings, treatment variables and measurement variables can the effect be generalized' (Campbell & Stanley, 1966, p. 5). However, the Campbell & Stanley (1966) article and also the later one by Cook & Campbell (1975) primarily examined the details of specific (experimental and quasi-experimental) research designs rather than providing an exhaustive discussion of validity concepts as such. Consequently, several issues regarding the unique implications and the interrelations of internal and external validity have not been clearly specified and have remained intuitive rather than grounded in an explicit analysis. Thus only cursory justification was brought in support of assertions regarding the logical priority of internal to external validity (Campbell & Stanley, 1966), the relations between external validity and representative sampling, the relative priority of external validity concerns in applied *v.* theoretical research (Cook & Campbell, 1975), and other issues.

For the last decade or so the internal–external distinction has dominated much of the discussion about validity problems in research designs. At the same time, alternative classes of validity concerns have become increasingly apparent. Thus Aronson & Carlsmith (1968) discussed the problem of 'multiple meanings' attachable to the operational realization of any conceptual variable. Cook & Campbell (1975) viewed the foregoing as a third separate category of validity threats (after those to internal and external validity), and branded it as the problem of 'construct validity'. Also, Cook & Campbell identified a fourth class of validity threats stemming from the statistical basis of research conclusions in psychology, specifically, the threat of erroneously pronouncing the existence or non-existence of an effect (i.e. committing a Type I or Type II error). This category of threats was referred to as the problem of 'conclusion validity'.

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Despite the useful distinctions just considered, the absence of an analysis relating them to more basic ends of scientific inquiry seems to have led to several confusions. Some of these have been problems of classification (e.g. the inconsistent labelling of specific threats as problems of internal or external validity). Others (e.g. misinterpretations regarding the requirement of representative designs) have led to erroneous recommendations or inappropriate criticisms levelled at the existing modes of psychological inquiry.

The purpose of the present paper is to provide a clarifying analysis of validity problems pertaining to the outcomes of experimental research. Toward that end, two fundamental concerns of a scientific inquiry will first be discussed, notably the problems of (1) context and (2) causal meaning attachable to the research results. Subsequently, an attempt will be made to show how several common misconceptions regarding outcome validity may be elucidated through a reduction to one of the two basic problems above.

THE CONTEXT AND MEANING OF RESEARCH RESULTS

Within any instance of scientific research the results obtained may be characterized in two significant ways: (1) in terms of their context, or scope of applicability, i.e. the boundary conditions beyond which they may not be expected to hold; (2) their conceptual meaning, i.e. their interpretation in terms of a (maximally) general law operative within the given context. In what follows the problems of context and of meaning will be considered in some detail.

The problem of context. Different research instances may vary greatly with respect to the nature and restrictiveness of the context within which their results are expected to hold. The specifications of context may derive from theoretical or practical considerations. Thus Brunswik's plea for the formulation of theories applicable within the *organism's natural ecology* is one example of a theory-determined delineation of context. Another such example would be an ethologist's theorizing uniquely applicable to a *specific species*. Alternatively, one might seek to evolve a theory of a particular '*system*'(e.g. international, economic, cognitive). In this case, restrictions upon the intended context of applicability would follow from the *unique* set of properties assumed to define the system at issue and set it apart from the other systems. In many instances of theoretical research in psychology the intended context may be broader and less explicitly stated than above, e.g. the results are implicitly assumed to apply to the category of all the organisms and within a broadly normal range of conditions. Restrictions upon the context of an inquiry may follow from practical as well as from theoretical considerations. For instance, one may wish to make inferences restricted in scope to a particular context of applied significance, e.g. a school, a factory, a community or a hospital.

In some cases the applicability context of a research proposition may be characterzied as variable with regard to the predicated phenomenon, while in other cases as invariable. For example, the context 'population of all American voters' may be characterized as variable with respect to the predicate 'votes Republican in the next election'. By contrast, the context of 'all living organisms' might be considered as invariable with regard to the predicate 'responds with aggression to frustration'. In the case of a variable context, the propositions rendered may only characterize the existing variability in terms of its parameters (e.g. indices of its central tendency). Such propositions constitute what Bakan (1955) has called 'aggregate-type' statements, i.e. ones pertaining to aspects (parameters) of the aggregate *as such* rather than to each component member thereof. In the case of an invariable context, the propositions rendered will pertain to the general rather than to the aggregate (Bakan, 1955), i.e. constitute statements about *each* member of the designated class rather than about the class taken as a collective.

Within psychological research aggregate-type propositions have been common in applied investigations, e.g. surveys intended to characterize finite, clearly delineated groups (e.g. the population of all American males in a study of sexual habits). In theoretical research, Brunswik stands out as a prominent advocate of aggregate-type propositions (or statistical laws) pertaining specifically to some summary properties of the collective of stimulus conditions constituting the organism's natural ambience. Note that despite the widespread use of statistics in theory-testing psychological research, most of the investigations in this category aim to expound general (rather than aggregate) statements.* To be sure, propositions such as 'partial reinforcement enhances the resistance to extinction' or 'anxiety leads to affiliation' are meant in reference to organisms or humans *in general* rather than to some impossible collective of all humans or organisms.

Whenever a proposition, supported within a research instance, fails to hold within its intended context of generalization we have what will henceforth be labelled as the case of *context invalidity*. In other words, context invalidity may be characterized as the instance wherein the context (or background) conditions in a piece of research are in some way *non-representative* or *dissimilar* with respect to the referent context. For example, in research about the aggregate of all Americans the use of a telephone survey (omitting Americans not possessing a telephone) may be *a priori* suspect of non-representativeness. Or one may find that a statement originally assumed applicable to the class of all humans holds only for people in culture A but fails to hold for people in culture B, in which case mutual non-representativeness of the two contexts (cultures) is inferred *ex post facto* i.e. after the results from both cultures were compared.

Note that the novel term 'context invalidity' denotes the issue of generalizability (of a proposition within its intended context), i.e. a topic for which the notion of external validity has long been reserved and widely accepted. The reason for this novel coinage is that the idea of external invalidity has been fully identified with a statistical interaction between the treatment (X) and the background of a research (Campbell & Stanley, 1966, p. 16). However, *context invalidity* constitutes but one sense of such an interaction. As will be shown in the following section, the X-by-background interaction has another important sense pertinent to the problem of causal meaning attachable to the research results. To the latter issue we now turn.

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* In a typical theory-testing study one applies the test of significance to the difference between the experimental and control means and the outcome distributions in the two groups usually exhibit a degree of overlap. Yet the researcher's intention is *not* an inference regarding these particular means, but rather about each case in the experimental *v.* the control conditions. Furthermore, the overlap in distributions is *not* interpreted as the operation of a statistical law, but rather as the existence of alternative determinants of the phenomenon under study randomly distributed as between the two conditions.

THE CAUSAL MEANING OF RESEARCH RESULTS

An elemental objective of science is the formulation of general laws. This means the explanation of the observed phenomena in terms of their most basic causes. The process of causal explication begins with the single experiment. In the prototypal case, subjects in one group are exposed to a treatment hypothesized to constitute one determinant of the phenomenon under study. Subjects in a control condition are submitted to a nearly identical set of circumstances, save for the treatment. The first precondition for causal explication is the existence of something to be explicated, notably a difference in results yielded by the two groups. Once such a difference has been observed there begins a successive purification of its causal meaning. The purification process is based on the identification of a possible cause of some special (e.g. theoretical or applied) interest, and the exclusion of plausible alternative causes for the observed difference. In what follows several classes of such alternative causes will be dealt with.

Conclusion validity. In most instances of psychological research the first alternative explanation of any effect has to do with 'chance', i.e. the hypothesis that the observed effect reflects the random combined operations of many unidentified determinants of the phenomenon, distributed in a haphazard fashion between the groups under comparison. A threat to the validity of the assertion that a systematic (*v.* a random) difference separates the comparison groups has been labelled by Cook & Campbell (1975) as the problem of *conclusion validity*. The common tests of significance yield P values which (if sufficiently small) provide the logical basis for an exclusion of the hypothesis of random differences. Once a systematic difference between the comparison conditions has been pronounced it is necessary to establish further whether it was indeed produced by the specific event(s) of experimental interest, i.e. by the experimental treatment. Accordingly, this problem will be referred to as that of treatment validity.

Treatment validity. There exist two categories of threat to validity of the proposition that the experimental treatment produced the observed effect: (1) the *main effects* of plausible causes *covariant* (or confounded) *with the treatment* (i.e. present in the treatment condition(s) and absent in the no-treatment condition(s)). Campbell & Stanley (1966) labelled this category of alternative explanations (consisting, for example, of historical trends, maturational processes, selection biases, etc.) as threats to *internal validity*. In fact these authors fully identified the concept of internal invalidity (i.e. main-effect-type threats) with the question of treatment efficacy. However, treatment validity may also be imperiled by (2) the plausible alternative causes implied in statistical interactions between the treatment and the background of a research. In other words, the result is present only under some particular combination of the treatment and the background and absent in other instances, hence such combination may be properly regarded as its cause.

In the preceding section a treatment by background interaction was treated as a problem of *context invalidity* (i.e. the failure to uphold a research proposition within its intended context of applicability). By contrast, the present discussion dwells on a different (and much neglected) sense of such interaction, notably the matter of causal meaning attachable to the observed effect. A concrete example may clarify the distinction between the two proposed senses of the X-by-background interaction. Consider an experiment on the effect of anxiety on performance conducted (incidentally) with a sample of slightly sleepy individuals. Suppose that this study has clearly shown that the quality of performance varies positively with the degree of anxiety. Suppose further that a replication experiment conducted with alert subjects finds the opposite relation, specifically a negative one between performance and anxiety. Under these circumstances, our researcher may adopt one of two possible tacks: (1) he may delimit the context of the proposition that performance varies positively with anxiety to one of slightly sleepy humans, and advance a second proposition, restricted to the class of alert humans, that performance varies inversely with anxiety; (2) alternatively, it may occur to our researcher that the unique combination of sleep and anxiety defines a novel, pertinent cause of the observed phenomenon, e.g. the level of general arousal and that the latter is related to performance in a non-monotonic fashion.

Whether one or the other tack will be pursued may depend upon their relative feasibility, i.e. which appears the more meaningful: the novel, sequestered context(s) or the novel, composite variable. In the example above, the suggested contexts (of sleepy or alert humans) appear of dubious significance, while the conjunctive variable of arousal seems quite meaningful. But in an interaction, say, between the subjects' sex and the method of practice (massed *v.* distributed), the separate contexts implied (males and female) seem readily palatable whereas it may be difficult to abstract a meaningful composite variable out of this interaction.*

Construct validity. Suppose that in a given research, treatment validity was satisfactorily established, i.e. it was shown that the experimental treatment alone (rather than variables confounded or interacting with it) was responsible for the observed effect. The next step in the progress of causal explication would need to clarify the nature of the constructs underlying the observed covariation between the treatment and the (dependent) measure. Thus plausible constructs (say A and B) are usually stipulated and it is then demonstrated that (1) the specific treatment aspect which embodies construct A (rather than other treatment aspects) actually covaries with the measure-aspect which follows from construct B; (2) alternative operational derivatives of constructs A and B (i.e. other treatments and measures) exhibit the same relation.

The progress of causal explication need not terminate with the establishment of a valid relation between some constructs. Rather, the latter may be assumed to represent more basic or higher-level constructs, and so on (e.g. see Turner, 1965, p. 178). In other words, a higher level construct is assumed to subsume several lower-level constructs in the same way as each lower-level construct subsumes several different operationalizations thereof. The hypothesis that higher constructs underlie the observed relations between lower ones requires the demonstration that all lower-level realizations of the supraconstructs exhibit the same pattern of relations, etc.

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* It will be noted that within theoretical research the separation of contexts may be regarded as a practical limitation resulting from insufficient knowledge. Within such research the relentless quest is for maximally general laws, so context-separation (i.e. a departure from generality) exists only as a temporary (even if sometimes a protracted) challenge to be overcome in the future by an insightful redefinition of the critical variables.

Note that each step in the explication progress just outlined constitutes the necessary condition for the next step. Thus, before attempting to establish conclusion validity regarding the presence of a *systematic* difference it is necessary to note the existence of *any* difference. Similarly, the pronouncement of conclusion validity regarding *any systematic* difference is logically prior to its attribution to the treatment variable within a given research. Finally, the establishment of treatment validity is logically prior to concern about the various levels of construct validity.

Let us turn now to consider how several prevalent confusions regarding validity concerns may be clarified by reference to the problems of context or meaning above.

AMBIGUITIES ABOUT VALIDITY CONCERNS AND THE PROBLEMS OF CONTEXT AND MEANING

(1) *Terminological confusions.* While not too serious in terms of practical implications, there seems to exist a nevertheless definite tendency for confused classification of the various validity types. Thus Cook & Campbell (1975) remarked on the frequent confusion by authors of internal and construct validity. Similarly, internal and external validities have sometimes been interchanged. For example, Rosnow & Rosenthal (1970) referred to a hypothetical interaction between the subject's volunteer status and an experimental treatment (e.g. a persuasion attempt) as a problem of *internal validity*, whereas Campbell & Stanley (1966) designated an interaction between an experimental variable and a selection bias (in this instance the volunteer status) as a threat to external validity. Also, Fishbein & Ajzen, 1975) classified the reactive effects of the experimental arrangements, and an interaction between a treatment and pretest as problems of internal validity, while Campbell & Stanley classified them as problems of external validity.

It is presently suggested that the inconsistencies just mentioned may reveal a reaction to the essential similarity of the various validity types as steps in the progress of causal explication discussed earlier. Such a reaction may well blur their distinctions within that progression. In particular, insufficient attention has been paid to the significance of the X-by-background interaction in the explication chain. This type of interaction has been generally assimilated to the notion of external invalidity and treated as a problem of generalizability. But, as noted earlier, the X-by-background interactions as well as main effects of confounded causes constitute the two categories of threat to treatment validity, i.e. the conclusion that the treatment may properly be credited as the cause of the observed effect. Thus reactive effects of the experimental arrangements, an interaction between a treatment and a pretest or one between a treatment and a selection bias may all be interpreted as suggesting a novel cause of the observed effect (e.g. the subject's hypothesis about the experimenter's purpose) alternative to the treatment. Take, for instance, the hypothetical interaction between the subject's volunteer status and the presence (*v.* absence) of a persuasion attempt. Rosnow & Rosenthal (1970) suspected that volunteers, presumed to have stronger affiliation needs than the non-volunteers, would show a greater degree of attitude change under persuasion. Clearly, the combination of volunteer status and persuasion attempt points to an important cause of attitude change, notably the degree of resistance to persuasion (assumed weaker in volunteers *v.* non-volunteers). This latter cause appears to provide a more cogent explanation of the results than the original treatment, i.e. a mere persuasion attempt. Possibly, in this instance, the authors branded an interaction between a treatment and a background factor (selection bias) as a threat to internal validity because, much like internal invalidity, it imperils the conclusion that the treatment as such caused the phenomenon of interest.

(2) *The roster of threats to 'external validity': a double entendre.* Particularly pertinent to the dual sense of a treatment by background interaction is the interpretation of threats to external validity listed by Campbell & Stanley (1966, pp. 5–6). These threats comprised: (1) 'The reactive or interaction effect of testing'; (2) 'The interaction effects of selection biases and the experimental variable'; (3) 'Reactive effects of experimental arrangements'; and (4) 'Multiple-treatment interference'. Consider the above factors in reference to the two senses of external validity discussed earlier. Specifically, each factor could be alternately regarded as: a threat to (1) context validity, (2) treatment validity. In the first sense, the presence within a research of such a factor implies that the result may fail to generalize to contexts from which this aspect was absent. But this does not mean the failure of generalization to all contexts. On the contrary, to the extent that one wishes to generalize to contexts characterized by the presence of the factor in question, its inclusion within one's research may well be regarded as a *source of strength* rather than as a *threat* to context validity.

In the second sense, the several threats listed may be viewed as general modifiers of the psychological meaning of experimental treatments so the causal interpretation of the results in terms of these treatments may be in error. For example, under reactive arrangements experimentally manipulated anxiety might assume for the subject the meaning of a challenge for a courageous self-presentation; consequently, any effect of such a manipulation may be more properly rendered in terms of this novel variable (i.e. challenge) rather than the original one (i.e. anxiety).

(3) *The question of priority as between internal and external validity.* Several authors (e.g. Campbell & Stanley, 1966; Aronson & Carlsmith, 1968; Cook & Campbell, 1975) have asserted the logical priority of a concern for internal *v.* external validity. The rationale for their position has been that it is first incumbent to demonstrate that a treatment had an effect before worrying about its generality. This implies that a concern for treatment-validity always takes precedence over a concern about context validity. But while such may be the case for much of theoretical research in psychology, wherein the conceptual purification of *causes* is the foremost objective, it is *not true* for wide varieties of research (e.g. of applied type) where the prime end is to produce the 'right' *effects*, i.e. effects which may be expected to obtain within a clearly designated context of some special interest. Consider, for example, a survey

attempting to characterize some behaviour of the average American. In such research, context validity concerns (e.g. a large and random sample) clearly loom important, while treatment validity concerns (e.g. what precisely has caused the behaviour in question) may be of little interest.

Or consider the simulation of a specific international conflict in which the question posed concerns the effect of some variable (e.g. a leader's personality) on the course of future events. Before attempting to test such a proposition one may require evidence of having simulated the right conflict. Such may be inferred from an isomorphism of outcomes between some real events within the system and the reproduction thereof within the simulation. In this case, too, concerns of context validity (i.e. an accurate representation of context) may be viewed as prior to concerns of treatment validity.

Finally, when external invalidity (an X-by-background interaction) is interpreted as an interactive threat to treatment validity there is little reason to regard it as secondary in importance to a main-effect type threat, i.e. internal invalidity.

(4) *Misconstruals of the representativeness requirement as a condition for context validity.* Campbell & Stanley (1966, pp. 23–4) implied that random (or representative) sampling contributes to the external (in the sense of context) validity of a research. In the same vein, Rosnow & Rosenthal (1970) regarded the possibility that volunteer subjects differ in several of their psychological characteristics from non-volunteers (thus being non-representative of the total population which contains both types of persons) as a significant threat to external validity. More recently, Cook & Campbell (1975) advocated random sampling from designated universes as a means of improving external validity, etc. On first glance the above statements may seem to follow from the notion (discussed earlier) that the failure of context-representation jeopardizes context validity. But it is important to point out that the requirement of random sampling (of subjects, situations, etc.) from some designated universe applies only to research testing *aggregate-type* (or statistical) propositions, i.e. statements about some parameters of that universe taken as an aggregate. But for the preponderant instances of psychological research concerned with general-type propositions random sampling is unnecessary.

To reiterate an earlier characterization, in an aggregate-type research the purpose is to test a specific proposition about a parameter of some collective (e.g. the average of all American voters). Under these circumstances the statements obtained within a research will be more exact (thus possessing a greater degree of external validity) to the extent that the research sample more closely approximates the referent aggregate, e.g. is larger and/or more randomly drawn. But in general-type research the proposition at stake is expected to apply to each and every member of the designated class. Hence, in such a research any member or a group of members is sufficiently representative. In other words, in general-type research random sampling or even the exhaustive containment within the research of some aggregate (e.g. population of all humans) does not safeguard the validity of the proposition at stake any more than does a small, non-random sample. For instance, even if a given proposition was shown to hold on the average within a large, statistically representative sample, it could still be falsified with any single member of the designated universe (see also Bakan, 1955, 1966; Kruglanski, 1973).

It is worth noting that the above criticisms of the randomness requirement do not vitiate the justifiable procedure of randomly assigning subjects (from any nonrandom group within the relevant context) to experimental conditions. In this case randomness is meant to ascertain (within the limits of sampling error) the comparability of subjects across the treatment condition, hence it bears on the issue of treatment validity rather than of context validity.

CONCLUSIONS

The main theme of the present paper has been that the several validity problems with respect to the outcomes of a research may be ordered by reference to two separate concerns: (1) the context of applicability of research results, and (2) their causal meaning. It has been proposed that the distinction between *internal* and *external* validities might be profitably replaced by that between context and *treatment* validities. *Context invalidity* has been defined as the failure of a proposition to hold within its intended context of generalization and has been characterized as *one sense* of a statistical interaction between the treatment (X) and the background of a research (formerly identified as external invalidity). *Treatment invalidity* has been defined as the question whether the treatment in a research may be credited with the observed effect. It has been suggested that treatment validity may be jeopardized by two separate categories of threat: (1) the main effects of plausible causes confounded with the treatment (formerly conceptualized as internal validity threats); (2) interactive effects of the treatment and the background, implying that the observed effect may be causally attributed to some composite of the treatment and background rather than to the treatment as such.

The problem of treatment validity has been presented as one segment in the chain of causal explication consisting of the establishment of conclusion, treatment and construct validities. Several prevalent confusions with respect to validity concerns have been discussed in light of the distinction, above, between the problems of context and of meaning attachable to the results of a research.

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REFERENCES

ARONSON, E. & CARLSMITH, J. M. (1968). Experimentation in social psychology. In G. Lindzey & E. Aronson (eds.), *Handbook of Social Psychology*, rev. ed., vol. 2. Reading, Mass.: Addison-Wesley.

BAKAN, D. (1955). The general and the aggregate: a methodological distinction. *Percept. mot. skills* **5**, 211–212.

BAKAN, D. (1966). The test of significance in psychological research. *Psychol. Bull.* **66**, 423–437.

CAMPBELL, D. T. & STANLEY, J. C. (1966). *Experimental and Quasi-experimental Designs for Research.* Chicago: Rand McNally.

COOK, T. D. & CAMPBELL, D. T. (1975). The design and conduct of quasi-experiments and true experiments in field settings. In M. D. Dunnette (ed.), *Handbook of Industrial and Organizational Research.* Chicago: Rand McNally.

CRONBACH, L. J. & MEEHL, P. E. (1955). Construct validity in psychological tests. *Psychol. Bull.* **52**, 281–302.

FISHBEIN, M. & AJZEN, I. (1975). *Belief, Attitude, Intention and Behaviour: an Introduction to Theory and Research.* Reading, Mass.: Addison-Wesley.

KRUGLANSKI, A. W. (1973). Much ado about the volunteer artifacts. *J. Person. soc. Psychol.* **28**, 348–354.

ROSNOW, R. L. & ROSENTHAL, R. (1970). Volunteer effects in behavioural research. In *New Directions in Psychology IV*. New York: Holt, Rinehart & Winston.

TURNER, M. B. (1965). *Philosophy and the Science of Behaviour*. New York: Appleton-Century-Crofts.

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RESEARCH NOTE: A NEW TECHNIQUE FOR STUDYING PHENOMENAL CAUSALITY

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The purpose of this paper is to describe a new piece of apparatus for generating stimuli for what Michotte (1963) called 'the perception of causality'.

Michotte's work has been described in many papers (e.g. Boyle, 1960, 1961) as well as in his own book, so the phenomena that he demonstrated need not be redescribed here. Most people are aware that the experiments conducted by Michotte and by his student Crabbé (described by the present author in a letter to the *Bulletin of the British Psychological Society* (**26**, 1973, p. 82)) as well as many others, were conducted with the disks apparatus. Whilst this technique has great flexibility, it has a number of disadvantages, particularly the fact that the stimuli change shape from rectangular to rhomboid when entering into movement, then revert to the rectangular shape on becoming stationary once more. Michotte devised an alternative form of apparatus using projectors, in which the stimuli were illuminated circles on a screen, but this apparatus was not very successful. The apparatus we are about to describe is based upon the principle of projected lights, but is designed with more up-to-date electronic principles than Michotte had in mind. It has been designed in our Departmental workshops by Mr J. Torrie and Mr L. Gray.

APPARATUS

Two slide projectors (Rank Aldis Tutor 2) are used. Each is fixed in position and projects a spot of light on to its own mirror, which reflects the light on to a screen by back projection (Fig. 1). The mirrors are made to swivel by a motor and gear box, the motors being electronically controlled.

By means of this system it is possible to cause one or two spots of light to move laterally across the screen, the total effective distance of travel being one metre. The absolute speed of travel, as well as the speed ratios of the two spots, can be readily altered by switches, as can the distance of travel of each spot. The maximum absolute speed is 25 cm/sec., the minimum 3 cm/sec., the range of effective ratios being from 8:1 to 1:8.

STIMULUS PRESENTATION

The subject sits 1·5 m in front of a white screen on which he sees two circles, each 18 mm in diameter. The one on the left is green, that on the right being red. Sometimes the positions of the colours may be interchanged, but in most of the demonstrations to be described the green circle moves first. The subject is asked to watch the screen and describe what happens.

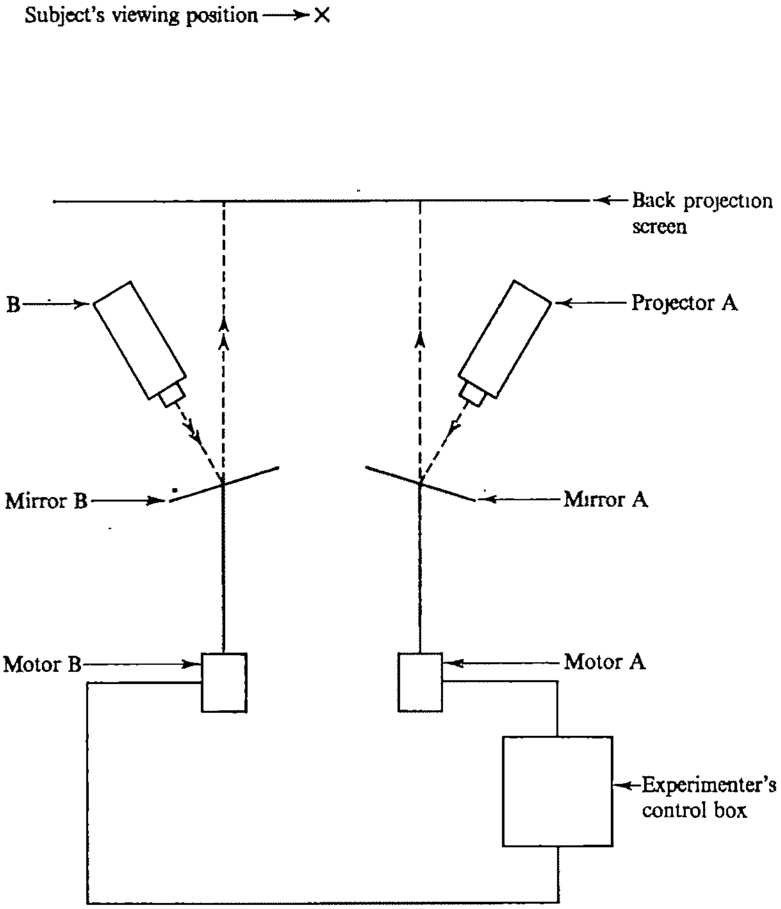

Fig. 1. Schematic representation of projection apparatus viewed from above (not to scale). (For description see text.)

PRELIMINARY FINDINGS

As this research is in its early stages, we have followed the Michotte plan of asking a small number of the laboratory staff (including secretaries and teaching colleagues) to observe the phenomena. The aim has been to see if the stimuli will elicit the characteristic Michotte-type responses indicating a causal impression. Preliminary results indicate that it is possible with this new technique to elicit responses indicating launching, triggering and entraining, as well as other impressions not described by Michotte, which we hope to report in a later paper.

Michotte's work has been criticized by Joynson (1971) on the grounds that his experiments were poorly controlled. However, as Boyle (1972) pointed out, Michotte's 'experiments' were really demonstrations: the French word 'expérience' may be translated as 'experience' as well as 'experiment', and Michotte's aim was to cause his subjects to experience certain impressions. Some critics have suggested that Michotte's results may have been an artifact of the disks apparatus, which is why it is of interest to discover that the same results may be obtained by a modified projection method.

REFERENCES

BOYLE, D. G. (1960). A contribution to the study of phenomenal causation. *Q. J. exp. Psychol.* **12**, 171–179.

BOYLE, D. G. (1961). The role of the 'radius of action' in the causal impression. *Br. J. Psychol.* **52**, 219–226.

BOYLE, D. G. (1972). Michotte's ideas. *Bull Br. psychol. Soc.* **25**, 89–91.

CRABBÉ, G. (1967). *Les conditions d'une perception de la causalité.* Paris: Centre National de la Recherche Scientifique.

JOYNSON, R. B. (1971). Michotte's experimental methods. *Br. J. Psychol.* **62**, 293–302.

MICHOTTE, A. (1963). *The Perception of Causality.* London: Methuen, 1973.

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BOOK REVIEWS

Francis Galton: the Life and Work of a Victorian Genius. By D. W. FORREST. London: Elek Books. 1974. Pp. xii + 340. £5.50.

It is surprising that we have had to wait for 45 years since the completion of Karl Pearson's massive three-volume biography of Francis Galton before the appearance of a second, and less unmanageable, account of his life and work. Professor Forrest has provided us at last with a much more readable, and very timely book – timely, because our present-day environmentalists have depicted Galton as 'the champion of the privileged classes', and ascribed to him 'an élitist philosophy'. Forrest's biography, though much less adulatory and much more critical than Pearson's, reminds us again that Galton was a man of unquestioned and varied genius, and, although he certainly had his prejudices, was far from being a defender of the Victorian Establishment. Indeed, his matter-of-fact inquiries into religious belief, his disparaging comments on the hereditary aristocracy and his liberalism in politics far from endeared him to the 'privileged classes'. The truth is that Galton was essentially and above all else an intellectual inquirer; a man with outstanding analytic gifts, who thought for himself about many questions no one else had ever systematically thought about before, including a good many questions in psychology. His views were shaped by what he regarded as the evidence, and he did at least take pains (unlike many of his critics) to collect evidence instead of merely opining. If he not infrequently oversimplified problems, this is hardly surprising. Every problem, in every science, is initially treated in a simplified way, and turns out after further investigation to be more complex than was at first supposed. So if Galton propounded simplified and even partly erroneous views of heredity, for example, and the measurement of ability, this is only to be expected and does not diminish his importance as a pioneer. Galton will live because of the creative intelligence he applied to a diversity of problems, and this is how he struck his contemporaries, even his left-wing contemporaries like Beatrice Webb, whose description (not quoted by Forrest) is worth recalling. 'Among these scientists', she writes, 'the one who stays in my mind as the ideal man of science is not Huxley or Tyndall, Hooker or Lubbock, still less my guide, philosopher and friend, Herbert Spencer, but Francis Galton.... Even today I can conjure up...that tall figure with its attitude of perfect physical and mental poise.... Fascinating to me was Galton's all-embracing but apparently impersonal beneficence. But to a recent and enthusiastic convert of scientific method, the most relevant of Galton's many gifts was the unique contribution of three separate and distinct processes of the intellect; a continuous curiosity about and rapid apprehension of individual facts, whether common or uncommon; the faculty of ingenious trains of reasoning; and more admirable than either of these, because the talent was wholly beyond my reach, the capacity for correcting and verifying his own hypotheses by the statistical handling of masses of data' (*My Apprenticeship*, pp. 134–5).

Professor Forrest's most interesting and often entertaining book is directed primarily to the general reader rather than to the psychologist. Of course, there are inevitably large chunks concerned with psychology, statistics and heredity, but there is lots besides – family affairs, travel and adventure, contributions to meteorology and geography, finger-prints, eugenics and a delectable appendix on Galton's many inventions, always ingenious, often amusing and rarely quite coming off! For Galton (and this is true even in statistics and psychology) never quite had the technical know-how to back up his brilliant capacity for innovation.

For those who stereotype Galton, the psychologist, as primarily a psychometrician, the extent of his introspective contributions may come as something of a surprise. Galton was, in fact, one of the great introspective psychologists of all time, capable in this very difficult ground of coming to original conclusions. In a letter to his niece he recommends the 'frequent withdrawal into one's self and looking at and directing one's own conduct as if it were that of an alien', and adds that at one time he used to overdo this practice himself. His introspective study of free-will has rarely been equalled, and he anticipated Freud by some years in his observations on dreams and the unconscious. 'It is certain', he wrote, 'that those who introspect, and those who study the genesis of dreams, succeed in discovering plain causes for numerous images and thoughts that had seemed to have arisen spontaneously': while free associations, he notes, 'lay bare the foundations of a man's thoughts with curious distinctness and exhibit his mental anatomy with more vividness and truth than we would probably care to publish to the world'. Dr Forrest suggests that Freud, though he does not mention Galton, may well have seen Galton's article in *Brain*, a journal to which Freud subscribed, and may have derived his free association technique from Galton. Ernest Jones, in his *Life of Freud*, attributes it rather to an earlier essay by Ludwig Börne. It is impossible to say who is right, and perhaps, as Dr Forrest observes, it does not much matter.

When he comes to statistics and heredity Dr Forrest gives us a critical appraisal of Galton's work, which recognizes its importance, but at the same time its mistakes and biases. He has had access to the extensive collection of material in the Galton Archives at University College London and also, of course, has been able to take advantage of subsequent scientific advances. Pearson had been too closely and too recently involved with Galton, and he had become too hotly engaged in controversy himself to be detached. Dr Forrest preserves an admirable detachment, without diminishing his own, or the reader's, respect for Galton's genius.

Psychologists will perhaps be disappointed that Dr Forrest did not devote more space to a critical analysis of Galton's contributions to psychology. There is still room for a book concerned primarily with these contributions. Forrest gives us short summaries, but has no space for a detailed and critical analysis. There is not much about Galton's all-important ideas on the nature of ability – his distinction between general and special abilities, which has underlain a good deal of subsequent work on intelligence; nor much on his really pioneering work on 'character' or personality assessment as we term it today. Nor is there a discussion of the influence of Galton's work on subsequent developments in psychology, or an analysis of some of its theoretical inadequacies. There is certainly room here for a deeper study.

Nevertheless, psychologists should be intensely grateful to Professor Forrest for this well-balanced and readable book. It has been well-produced and well-written, and my only complaints relate to the bibliography, notes and index. The bibliography of Galton's writings (15 pages long) is poorly laid out, and difficult to consult. The chapter notes annoyingly do not contain the chapter titles, so (unless one happens to remember the number of the chapter one is reading) one has constantly to refer back to the beginning of each chapter in the text. The index is thin, and it is hard to find passages one wants to refer back to. Perhaps these minor annoyances could be put right when a second edition appears.

L. S. HEARNSHAW

Brains and Reasoning. By GERHARD D. WASSERMANN. London: Macmillan. 1974. Pp. xii + 169. £6.50.

Dr Wassermann aims to establish a large and controversial claim: that philosophy of science will be able to get a deep understanding of the nature of scientific theorizing only if it makes use of neuropsychological hypotheses. About the only argument given in favour of this is that logical justification alone cannot support the reasoning involved in scientific theorizing because of the use of approximation procedures at all stages of scientific research, from deciding what follows from a given hypothesis to actually assessing how well such deductions agree with experiments. Decisions which cannot be reduced to formal logic thus have to be made by scientists, and Dr Wassermann thinks that neuropsychology is needed to illuminate what goes on here. Unfortunately, apart from sketchy remarks towards the end about various potentialities of brains (e.g. in thinking and in recognizing patterns) there is little indication in the book as to what this amounts to in practice, and what there is gives no support to the substantive view that neuropsychology can illuminate the *philosophy* of science. Although he realizes that there is no sense in which a brain model or an explanation of brain activity can help us to decide on the *correctness* of particular evaluations or calculations made by the owners of brains (p. 132), Dr Wassermann frequently misses the distinction between the logic or criteria by which judgements are made and the neuropsychological mechanisms involved in actually judging. It is this distinction which enables us to say that two theories may be incompatible even though no one has realized this (*pace* pp. 45–6) or that logical truths are independent of empirical systems (*pace* p. 14). Of course, empirical systems are involved in the recognition of logical truths or of inconsistency but the criteria for such judgements can be sharply separated from the mechanics of making them. From the point of view of philosophy, the judger is necessarily to be regarded as a black box (*pace* p. 53), because what is of interest is the judgement and the criteria involved, not the judger. Dr Wassermann is right to think that the logic of science cannot be reduced to formal deductive logic, but this does not show that there are *no* epistemological criteria for scientific judgements, let alone licensing a return to psychologism.

Much of the early part of the book is an attack on analytical philosophy, but Dr Wassermann's continued pressing of sceptical positions – he denies, for example, that we can say absolutely that anything is real because all we have is our own human perceptions of those things, or that we can ever say objectively that someone else is in pain because we cannot feel their pain – shows that he has not grasped the point of analytical philosophy in these and similar areas. This is to show that what the sceptic demands (a transcendental assurance of the reality of what I perceive or to feel someone else's pain) is unattainable and not required in order to speak of something as real or of someone else being in pain, and that we have in our experience all we need to make the relevant distinctions (between the real and the imaginary, the genuine and the feigned pain). Finally, Dr Wassermann's talk of brains rather than people judging, criticizing, having prejudices, etc., would be merely grotesque if it did not constantly lead him into howlers, such as saying that words refer not to things but to individuals' brain representations (p. 12) (so he and I do not speak of the same thing when we speak of Wittgenstein), or that concepts are applied to objects by brains comparing the objects to engrams (p. 143) (shades of Plato's third man) or that human speech activity symbolizes cognitive representations by brains (p. 3) (which will take us straight to all the problems of a logically private language).

ANTHONY O'HEAR

Behaviour: the Control of Perception. By WILLIAM T. POWERS. London: Wildwood House. 1974. Pp. 296. £5.50.

According to the dedication this book took 15 years to write; somewhere in the text it mentions 20. I am not surprised. It is bound to take time to model human behaviour from neurophysiology through to social ethics in terms of negative feedback in 296 pages. One way out, of course, is to select one's facts and this has been done with little finesse.

There is much to criticize in this book – the margins of my copy are littered – but there is also something to be gained. It is a pity that the reader has to work so hard through its awkward syntax and poor intra-chapter organization to reach the kernel. (The recurring tendency to use 'one' followed by 'he' to refer to the same person I found particularly jarring. Eventually this construction did cause semantic confusion with the sentence. 'One must first be sure he has correctly defined the controlled quantity.') The publishers naturally describe the book as being 'written with verve and wit' and one of the reviewers as 'in an easy and personal tone'. I failed to note either property.

It is also a pity that the book did not emerge 10 or even 20 years earlier, when negative feedback was in vogue and when it was an exciting idea that the secrets of human behaviour might yield to models involving interacting hierarchical mechanisms. Now it seems dated. Dated also is its extravagant coverage reminiscent of the aspirations of the early cyberneticians, and its choice of behaviourism as the straw man.

The central idea is that behaviour is subserved by nine (seven plus two?) hierarchically arranged levels. Each level receives input from the lower one, compares the input with a reference signal from a higher level and computes an error. Essentially, the purpose of the system is to control the error, which is caused initially by an environmental disturbance (the stimulus). The goal of the system is to reduce error to zero, at which point one's perception has come under control. I do not see this latter point as clearly as the author would like and feel some sympathy for the students who find the notion difficult (p. 251). As information proceeds through the levels, so degree of conceptualization increases. At the top resides motivation, and since it too must operate by negative feedback, a reference signal is needed which is supplied by a genetically determined mechanism signalling bodily needs.

If the treatise had stopped here all might have been well, since the system seems viable, probably even largely true (although I suspect that the reinforcing effects of saccharine solution would cause some trouble). But the author attempts too much. The first signs of trouble emerge with a sketchy attempt to account for social motivation, but there is deeper trouble ahead.

The chapter on memory begins with RNA and makes some interesting points on the physical distribution of memory in the brain and begins to probe possible retrieval mechanisms. This would have been most interesting if pursued but Powers is led instead to speculation in a region well studied by contemporary cognitive psychologists. Statements like 'I know of no systematic studies of hierarchical relationships among kinds of memory' (p. 210) and '*all* [his italics] behaviour consists of reproducing past perceptions' (p. 217) are to be found between 'wise' comments which a researcher in the area would feel reluctant to make. However, he admits disarmingly (p. 215): 'I am not familiar enough with the literature on memory to know whether experiments bear out these suppositions.' Having watched the torments of those attempting to bring order to quite restricted areas in human memory over the past 20 years, one can perhaps be excused the comment that this chapter is not going to help much. Nor, I feel, is the final chapter on social control.

Here Powers applies negative feedback to social issues, having obviously taken umbrage at Skinner's recent writings. The main thesis seems to be that people ought not to try to control other people. In a section embarrassingly entitled 'Freedom and Dignity' he joins Skinner's critics, but the commentary is not really original. It seems that people do not like other people controlling their 'intrinsic errors' by applying incentives to help resolve conflicts. Well, perhaps the welfare state is not going to work after all.

Powers seems to take it for granted that his system will come under experimental scrutiny by other workers. I rather doubt it, at the psychological level at any rate. The system has that capacity to be both wide and narrow simultaneously, so that even if a clear prediction should emerge a negative finding could easily be accounted for with additional assumptions. In any case it is surely up to the formulator to attempt verification. But for a 'Rat Experiment' (the author's words, not mine) we have to rely upon introspection, demonstration and anecdote.

The final comment should come from Powers himself (p. 224): 'Language is marvellous in the way it permits us to pass over difficulties without a ripple. Note how skilfully I bypassed the question of *what* flips the memory switch. "One" flips it! I plead guilty to obfuscation – the model obviously lacks some details which I am not now prepared to supply. I will continue to obfuscate, having no present alternative.'

D. W. J. CORCORAN

Psychotherapy and Behaviour Change, 1973. Edited by H. H. STRUPP, A. E. BERGIN, P. J. LANG, I. M. MARKS, J. D. MATARAZZO and G. R. PATTERSON. Chicago: Aldine. 1974. Pp. xiv+543. Institutions, $20.00; individuals, $14.00.

This volume, published in August 1974, consists of 44 papers selected from 22 British and U.S journals of 1973. It is the third volume on the topic published by the company who regularly publish similar selections on other topics. Inevitably when papers have to be selected almost as soon as they have been published there can be little time to assess the response of the professions to them and one is therefore dependent on the experience and acuity of the editors. A glance at the names listed above should reassure the reader on these counts.

The preface makes it clear that the editors have aimed to be eclectic. They reject any narrow or parochial point of view: 'Progress in the area has been seriously impeded by protracted acrimonious debates between and within theoretical orientations.' They praise the increasing tolerance of dissident points of view coupled with a questioning of accepted practices and theories. In addition to review articles there is in the selection a praiseworthy emphasis on research and statistical analysis. Six major sections are labelled: General Issues, Therapeutic Processes, Conditions and Outcome, Desensitization and Flooding, Behaviour Therapy: Adaptive Response Training, Group, Marital, Family and Encounter Therapies, Psychoanalytic Therapy.

In the first section appears Rosenhan's provocative paper 'On Being Sane in Insane Places', which on publication evoked much popular attention. It describes how eight sane people were admitted to and detained in a total of 12 mental hospitals for an average of 19 days after complaining that they had heard voices which seemed to say 'empty', 'hollow' and 'thud'. They admitted to no other symptoms or previous psychiatric history. Rather amazingly, although 30 per cent of the other patients saw through the pseudo-patients, none of the staff did so, with the result that the 11 who had been labelled 'schizophrenic' on admission were labelled 'schizophrenic in remission' on discharge. A paper replying to Rosenhan's conclusions is also included in this volume.

In the second section, there is a good critical review by Wilkins on patient expectancy, the psychotherapy equivalent of placebo effect. This reveals that there has been no valid experimental demonstration of the effect. A common fault in the studies yielding positive evidence was that they were not double blind, thus confounding patient and therapist expectancy effects. There is also a theoretical difficulty in separating expectancy effect from both feedback effect and from the patients' accurate prediction of outcome. The author concludes that expectancy effect is not a useful explanatory construct and that research should be focused on techniques and observable events which may be relevant to therapeutic outcome.

Many of the papers deal in a useful and interesting way with either standard or more recent techniques, including group therapy (four papers), assertive training and flooding (two papers each) and the treatment of delusions (Watts and Powell), obsessive-compulsive disorders (Marks), covert sensitization and sexual problems (two papers). Sobell and Sobell describe their work which compared the two goals of abstinence and control drinking in two controlled trials. The decision as to which goal was to be aimed for was made by the patient and staff, but then allocation to aversion therapy or conventional hospital therapy was made randomly. For each goal, aversion therapy was better than control treatment. For the two aversion treatments there was no difference between the rates of relapse into uncontrolled drinking, about 25 per cent at six months' follow-up for each, though there was a higher abstinence rate for the group who had had that goal than for the other.

Five of the papers in the last section are reprinted from the number of the *International Journal of Psychiatry* which dealt with the final report on the Menninger psychotherapy research project which had lasted 18 years and dealt with 42 patients. The summary and conclusions of the report are reprinted, together with three papers discussing the report and a reply from Kernberg, its author. Among the findings of the project is that high initial ego strength and high initial manifest anxiety were good predictors of improvement, irrespective of the specific psychotherapeutic technique used. Expressive psychotherapy which places most emphasis on here-and-now transference was the treatment of choice for patients with low initial ego strength. It also confirmed that a skilled therapist was more effective in all types of treatment than the less skilled and that the less skilled therapist contributed most effectively in expressive psychotherapy. Both May and Miles in their comments refer to the fact that assignment to type of treatment was according to preconceived ideas on the patient's suitability. Nevertheless, both praise the effort put into the project, particularly in devising quantitative measures and statistical approaches which will contribute to future research in this field.

I am sorry I have no space to describe many more of the papers. For the cost of this book there must be few journals which would provide the quantity of papers included here and no journal could hope to supply such a high proportion of very readable and useful ones. I can therefore recommend it as good value for money in these inflationary days.

RALPH J. MCGUIRE

Sight and Mind: an Introduction to Visual Perception. By L. KAUFMAN. New York: Oxford University Press. 1974. Pp. xii+580. £8.75.

The author's aim in writing this book was to produce a well-balanced and self-sufficient introduction for an undergraduate course in visual perception. At the same time he wanted to portray the subject as a living science rather than as a catalogue of dry facts. So he decided to present findings within their appropriate theoretical context, in the belief that theories not only serve to 'motivate experiments' but also to 'orient the student'. It goes without saying that any textbook which succeeds in covering such a vast field in reasonable depth, yet manages to 'orient' the student and make the subject come alive for him, would be welcomed by undergraduates and their tutors.

Is *Sight and Mind* such a book? Certainly it makes a good start by having a title that will evoke an orientation reaction from psychology students. But if this should lead them to expect a broader psychological treatment of the subject than is available in other texts, they will be disappointed. For this is essentially an introduction along fairly conventional lines, in which visual perception is considered largely in isolation from other psychological processes, in a series of more or less self-contained chapters, each dealing with the facts and theories specific to a particular area. And, as with most other textbooks, there is a rough progression from the sensory to the 'higher' levels of perception: from sensitivity to light and colour, through contrast, contour and flicker, to the classical problems of depth, size, movement and form.

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However, *Sight and Mind* is unusual in having an introductory chapter devoted to a discussion of some of the main points of view underlying research on perception, including the author's own. This he describes as being consistent with a form of behaviourism: 'perception is an active process of monitoring one's own behaviour'. Unfortunately, the discussion of alternative approaches, including the author's own particular variety of constructive theory, is too sketchy to enable the student to get any firm bearings in the field. But at least the opening chapter is readable and likely to whet the student's appetite for the subject, which is more than can be said of chapter 2 on 'Light and the Visual Pathways'. This begins with a dreary catalogue of dry facts – about bolometers and photometers, foot-lamberts and nits; and ends with an account of the anatomy and physiology of the retina and visual pathway so sketchy and superficial as to be of no real value to the student. Presumably it was included as part of the author's mistaken attempt to make the book 'self-sufficient'.

Although the next four chapters (part II: 'Light and Colour') are more readable, the curious student of perception will be left wondering what all this detail adds up to. What can we expect to learn about our perception of the world of objects and events from responses to isolated spots and flashes of light? Unfortunately, the author never raises general questions of this kind, but seems to assume that the significance of this 'basic information' will be intuitively obvious to the student. Intuiting that the study of flicker is of 'central importance to perception', he will readily submit to a course of instruction (pp. 97–109) in 'linear systems analysis' in order that he might better appreciate the finer points of contemporary flicker theory. Having cheerfully worked his way through flicker, he will be no less willing to tackle 'contrast and contour', processes which are obviously 'basic to form and space perception'. But he will feel cheated when he discovers that no real use is ever made of all this 'basic information' in the chapters on space and form perception (parts III and IV).

Despite the lack of continuity, he will nevertheless find a number of lucid and interesting chapters in the second half of the book. In particular, the chapter on binocular stereopsis, a subject to which Professor Kaufman has made important contributions, provides a comprehensive guide to the research literature, while chapter 13 gives a very readable account of the novel and promising approach to the problem of form perception in terms of the concept of 'spatial frequency'. These and other chapters in parts III and IV provide well-balanced introductions to specific areas of form and space perception and could be read with profit and enjoyment by undergraduates in psychology. But the lack of continuity between chapters and the very limited amount of discussion of the broader psychological issues makes the work as a whole unsuitable as a textbook, at least for students of psychology. It seems more appropriate for undergraduates in 'visual science'.

G. W. GRANGER

Social Communication and Movement. Edited by M. VON CRANACH and I. VINE. London: Academic Press. 1973. Pp. ix+489. £8.50.

The title of this book is misleading in that it is really non-verbal face-to-face interaction which is under consideration. The volume contains a diverse collection of papers which were, essentially, the contents of two symposia on non-verbal communication held in 1969. The symposia were primarily intended to bring together Europeans working on various aspects of non-verbal communication, and to this extent the representation of work in the field is limited. In a long introduction, the editors first do an excellent job in attempting to draw together the loosely related contributions, and then go on to present their ideas of the requirements for a comprehensive theory of communication. Indeed, the introduction is one of the most interesting contributions in the book.

As in all such volumes, chapters range in character from scholarly treatments of a topic (e.g. Vine), to discussions of specific research (e.g. Eibl-Eibesfeldt), to primarily methodological contributions (e.g. Frey and von Cranach). In a further attempt to lend coherence to these varied chapters, the contributions have been classified under three headings: I, Behaviour Patterns and Communicative Consequences; II, Structural Analysis of Facial Expressions; and III, Concepts, Strategies and Methods. Although one appreciates the editors' concern with giving structure to the book, these divisions are not entirely satisfactory, and may in fact draw more attention to the lack of unity in the collection.

In part I, Kendon deals with postures and groupings of individuals in communication and with synchrony of action patterns amongst communicators. Thus the research admittedly is more concerned with external organization of people in communication than with the communicative function, if any, of the behaviour observed. Next, in the one chapter dealing with communication in chimpanzees, van Hooff provides a comprehensive description of postural, facial and auditory signals, and comes closest of all the chapters to being related to the book's title. Some of the problems of moving from description to quantification are apparent in the attempts to establish interrelationships between behaviours and to reveal higher-order categories of behaviour. A particularly interesting contribution is made in Eibl-Eibesfeldt's description of expressive behaviours of deaf-and-blind-born children. Many of these behaviours closely resemble normal social responses, e.g. pouting, smiling. This fascinating descriptive and anecdotal material will no doubt engender further systematic investigation to shed light on the possibility that certain motor patterns used in social communication are not in the first instance learned. Vine nicely shows the interrelationship between perception–cognition and expressive behaviour in early human social development. To do this he covers a vast literature, thus providing a valuable bibliography of research up until 1971. In the rapidly expanding field of infant development some of the statements are necessarily out of date. Hindmarch deals with the importance of eyes in animal communication. He draws on phenomena such as eye spots in lower species, and pupil size and direction of gaze in humans.

In part II, the relationship between emotion and expression is explored in a brief chapter by Frijda. After setting out his conceptual analysis, he presents data on facial expression which he interprets as suggestive of an hiearchical structure underlying expression and emotion. In Stringer's chapter on judgements of facial expression, the problem is put in historical perspective, and the interplay between methodology and conceptualization is nicely shown. This chapter stands out as being a clear-sighted and broadly based treatment of the topic of facial expressive behaviour which is both critical and constructive.

In part III, Frey and von Cranach present a method for measuring 'variability of movement behaviour' (which will not be understood by all readers!). Empirical data bearing on individual differences in movement behaviour and effects of various experimental manipulations on body movement are presented. Next follows a chapter by von Cranach and Ellgring in which the problems of determining gaze direction are detailed. The final chapter by Timaeus relates to non-verbal communications in the experimental situation which may affect results. Research strategies to deal with experimenter expectancy effects are offered.

At the end of the introduction the editors say that from their point of view 'most of the critical research on the human communicative behaviour system has yet to be done'. As a sampling of kinds of approaches, methodological problems, and examples of interaction between methodology and conceptualization, this volume should be of particular use to students developing research interests in social communication.

G. C. CARPENTER

Social Disability: Alcoholism, Drug Addiction, Crime and Social Disadvantage. Edited by DAVID MALIKIN. London: University of London Press. 1974. Pp. xi + 266. £5.50.

This book is intended as a text to be used for training courses for social workers and others involved in the rehabilitation of sufferers from the social disabilities of the title. It is difficult to understand why the book was published in this country because the topics are discussed exclusively in the American context.

Each of the topics is described by more or less distinguished contributors and more or less to the point. The chapter on crime is written by the equivalent of a prison governor and is replete with sentiments of the 'criminals are human beings who are motivated by the same needs as everyone else' variety. Appropriate case histories are provided, followed by 'research and rumination' chapters which vary greatly in quality and coherence. The only contribution of more than parochial interest is that on alcoholism by a very distinguished worker in that field, Selden D. Bacon, but his views may be read elsewhere.

The only interest of the book for this reviewer lay in thinking about the reasons for the use of the term 'social disability'. In the introduction the editor tells us that the term identified Americans who find themselves the 'outcasts of society'. The reader has yet to discover that 'social disadvantage' is used to describe the situation of Black Americans who are almost always poor. It seems that vocational rehabilitation, by preparing the socially disabled for 'suitable gainful employment', will solve their problems and enable them to achieve 'an independent need-satisfying life style'. Perhaps this is an improvement on the notion, which may still be current somewhere, that family casework and individual counselling will cure all ills.

There is considerable discussion amongst sociologists on the assumptions implicit in the use of such terms as anti-social behaviour or conduct disorder or social pathology to describe and to prescribe responses to people who have come to notice because they are considered to have deviated from some currently acceptable norm of behaviour. The term 'deviance' has been proposed as the most theoretically neutral to describe, for example, the behaviour of individuals whose careers of alcohol consumption or use of certain drugs or breaches of the criminal law have qualified them for some kind of social response. It remains difficult though to consider ethnic origin as deviance let alone as a social disability or indeed as anything that requires a particular set of special social responses.

JOY MOTT

The Measurement of Intelligence. By H. J. EYSENCK. Lancaster: Medical and Technical Publishing Co. 1973. Pp. xii+488. £8.50.

Any book ought to be reviewed in terms of what it is trying to do rather than what various people might think or wish it was doing. This is particularly true in the area of intelligence, where emotive reactions frequently supersede rational analysis. Professor Eysenck states quite clearly that 'This book deals with one aspect of the modern scientific study of intelligence, namely its measurement.... We have not dealt at all with another aspect of modern intelligence testing which has attracted more and more attention in recent years, namely the social, ethical and political side.' In my view, the job it set out to do, it has done extremely well, and it will undoubtedly take a place in the literature as one of the most comprehensive and worthwhile efforts at discussing the history, theory and problems of the concept in measurement of intelligence.

The book itself is in 10 parts. Within each, representative articles of historical importance as well as more recent research are reproduced. Above all, the discussion throughout the text provided by Professor Eysenck illuminates both the problems and some solutions in this area. It is not necessary, in a review, to list all the articles, but readers will perhaps find it useful to know that the areas covered are as follows: History and Definition of the Concept; Measurement and the Problem of Units; Development and Constancy of the IQ; Types of Intelligence; Analysis of IQ Performance; Heredity and Environment I; Heredity and Environment II; Intelligence and Social Class; The Biological Basis of Intelligence; The Paradigm and its Critics. Though everyone reading a book with selected readings as a major part of it tends to feel that something they know about or have read has been omitted, it would, in this case, be difficult to identify any major article or contribution to the concept in measurement of intelligence which is not adequately represented in this text.

Particularly important in this text are the evaluative comments of the editor in terms of the methodology used in psychometric research investigations. In the final chapter, Professor Eysenck, perhaps more clearly than ever before, points out the scientific nature of psychological measurement and compares it with the methodology of laboratory experimental psychology. A number of recent authors have suggested that the so-called incompatibility between experimental psychology of cognitive functioning and the psychometric approach to measurement in this area are, indeed, not only compatible, but vitally interrelated both in theoretical and practical terms. I would hope that particularly chapter X would lead to more cooperation between all psychologists interested in the analysis of human behaviour by whatever theoretical model or practical approach.

In general, then, I regard this as an invaluable book which it was a pleasure to review, and which should be read, or at least referred to, by all psychologists whatever their major area of interest. I therefore recommend this book as one which will have a lasting place in psychology. Above all, it stresses the need for a scientific approach by psychologists to the understanding of human behaviour, irrespective of the area of study – intelligence, information processing or social interaction. Ethical and emotional considerations, though vital factors, must be applied to factual data and sound theory if humanity is to benefit from psychology.

R. D. SAVAGE

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Intellectual Functioning in the Aged. By R. D. SAVAGE, P. G. BRITTON, N. BOLTON and E. H. HALL. London: Methuen. 1973. Pp. 190. £4.85.

If, one day, consumer 'protection' is extended to the purchaser of books, two charges could be levied against the publishers of this volume. Both concern the description of the product. For *Intellectual Functioning in the Aged* is a monograph rather than a 'Manual of Modern Psychology' and the 'Intellectual Functioning' of the title relates mainly to performance on the Wechsler Adult Intelligence Scale. The book reports the results of a number of studies carried out in Newcastle upon Tyne during the period 1963–71 in which normative data on the full scale WAIS were collected for people over the age of 65. Some of the subjects completed a short form of the WAIS and in addition some were examined on the Modified Word Learning Test, the Paired Associate Learning Test and on a learning test based on block designs. The authors present WAIS results for two samples of people living in the community and for a hospitalized group. They also examine such important questions as the relation between WAIS performance and survival over a seven-year span, longitudinal change over this period in normal and psychiatric groups and the factor structure of the test scores. The book is thus a welcome addition to the scanty literature of longitudinal studies of ageing.

The projects described were part of a larger survey on mental illness in the aged population and the methods used ensured that samples I and II were reasonably representative of old people living in the community. The hospitalized sample, however, was a selected one, chosen to exemplify certain types of psychiatric conditions. A psychiatric diagnosis was obtained for subjects in all three samples but the labels applied differ from those in the literature cited as relevant and the authors do not comment on the fact that only about half the subjects in the community samples were considered 'normal'. No information is presented about the mean ages of the various diagnostic groups of sample I but subjects over 75 obtained lower scaled scores than the under-75s and, contrary to expectation, the older group had a lower discrepancy between verbal and performance IQ than the younger group.

Few surprises arise from the scores of the hospitalized group: the psychiatric groups had lower scores than the normals but did not differ markedly from each other. There is no information about what statistical tests were used in these comparisons.

This lack of information about the analyses undertaken is evident too in chapter 5, 'The Development of Intellectual Functioning'. This chapter deals with the longitudinal study of the combined samples over a seven-year period. Whilst one may sympathize with the authors when they say 'We do not feel it necessary to present the minutiae of each phase of the investigation...' it is surely not sufficient for them merely to assert that analyses of variance and covariance were carried out 'as appropriate', since it is not even clear what factors were considered in their design or what variables held constant. One may quarrel too with the combination of the disparate samples, since the different contexts of initial testing, differential drop out rates and heterogeneous diagnostic groups are likely to cloud the issues – particularly since main effects only are reported. Fortunately, Savage *et al.* present additional descriptions for the separate diagnostic groups. Surprisingly, the 'normals' decreased on verbal but increased on performance subtests over the seven years whilst the schizophrenics (a younger group initially) showed little change. In 'normals' verbal scores were related too, to subsequent survival.

Normative data on the elderly are hard to come by and on this count alone the book deserves a place. However, it is a book of frustrating inconsistencies and the standard of presentation is poor. There are many errors in tables and in the text, some of them important. Names are misspelt and age groups given incorrectly (e.g. 60–90, for 60–69). Moreover, the approach to intellectual functioning seems excessively narrow. During the seven years of the study the subjects must have lived lives of sickness or health, of interests or apathy, all of which might affect the level of intellectual functioning. But throughout this monograph there is no hint that the testers even chatted with those they tested.

ANN D. M. DAVIES

The Measurement and Classification of Psychiatric Symptoms. By J. K. WING, J. E. COOPER and N. SARTORIUS. London: Cambridge University Press. 1974. Pp. 233. £4.70.

There is a fashionable trend in some circles, both inside and outside psychiatry, to decry the use of any kind of diagnostic label as damaging to the dignity of the individual. While this may have served a useful purpose in preventing a stereotyped approach to patients, it has greatly increased the difficulty of evaluating any epidemiological studies. The wide discrepancies, for example, between the figures for the incidence of schizophrenic illness in the United States and the United Kingdom have led to many explanatory theories. Yet these theories may well be valueless if the actual diagnostic label is being applied to different illnesses.

This book is an attempt to summarize the findings of ten years' concentrated effort by a team of investigators whose aims are stated as devoted mainly to two questions. 'First, whether certain psychological and behavioural phenomena which have generally been thought by psychiatrists to be "symptoms" of mental illnesses, can reliably be recognized and described irrespective of the language and culture of the doctor or patient: secondly, whether rules of classification can be specified with such precision that an individual, with a given pattern of symptoms, will always be allocated to the same clinical grouping.' The present state examination (PSE) is a structured clinical interview designed to investigate in depth the patient's mental state and to rate the symptoms in a standardized way which will be comparable with similar interviews conducted by other clinicians in other places. To be sure whether the patient has a particular symptom, the clinician asks a series of questions and only if he is satisfied that it is present is a positive rating made. Simple affirmation by the patient is not enough. The PSE is thus in no sense a questionnaire but a flexible interview, much like that carried out in ordinary psychiatric practice. The use of the symptom ratings in a computer program CATEGO is also described. It is intended to aid in the classification of the disease which may be of particular value in research projects.

The early chapters are devoted to acquiring the data according to a clinical plan and chapter 5 discusses the reliability of the process. Chapters 6 and 7 deal with the computer program CATEGO and chapter 8 with the limitations and the uses of both of the PSE and CATEGO system.

This is an excellent book; it is scientific, scholarly and modest. At every step the limitations and possible faults of the approach are made clear to the reader. Although a little intimidating in its layout in parts, it is worthy of close study. For the clinician who is anxious to standardize his interview techniques for research purposes it will be instructive, as it will be to the clinical teacher for whom it offers an opportunity to review phenomenology. In fact, to anyone who is interested in the more scientific development of psychiatry, this book will be an inspiration both because of its basic concepts and their careful exposition which is so clearly the result of patient and meticulous work.

ISABEL C. A. MOYES

The Functions of Sleep. By E. L. HARTMANN. London: Yale University Press. 1974. Pp. ix+198. £3.50/£1.25.

It is probably true that any research field, viewed at most points in its development, abounds with direct contradictions of results and inconsistencies in data. With progress, such discrepancies may be resolved in a variety of ways; for instance, by more meticulous experiments or by drawing attention to vague assumptions, definitions, etc. Yet for the theorist concerned with explaining findings as they appear to him at the time of writing, the advantages of such hindsight clearly are unavailable. In such a position he may either elect to pursue a painstaking analysis of conflicting results, a strategy which by no means guarantees any resolution of the problems, or he may chose to take a selective sweep at the facts, picking up only those which lie along his theoretical path.

In this well-written book, Professor Hartmann adopts the second approach, openly admitting in the early sections that he intends to emphasize what to him are the most important findings and to leave out some lines of inquiry if they are not relevant, in his eyes, to the question of function. This is immediately disquieting. Those of different orientations will have grounds for questioning the omission of some research data, while students hoping to gain a comprehensive view of sleep function will be uncertain as to whether anything potentially important is being ignored.

After a brief survey of earlier theories and an overall sketch of well-established facts, Hartmann draws upon selected evidence from studies of sleep deprivation, individual differences in sleep profiles, neurophysiology and chemistry, to derive a theory of sleep function. In this he emphasizes the division of sleep into D-sleep (desynchronized sleep) more widely known as REM sleep, and S-sleep (synchronized sleep), the equivalent of non-REM sleep. He contends that slow-wave sleep, the more intensive part of S-sleep, has a physically restorative function in which macromolecules – proteins and/or RNA – are synthesized, especially in the central nervous system. This type of sleep plays a preparatory role for the functions of D-sleep which are manifold, but ultimately involve restoring catecholamine-dependent systems. D-sleep, he argues, therefore provides recuperation for systems involved in learning, memory, attention and emotional equilibrium during waking.

In arriving at this theory, Hartmann encounters many difficulties. It may be, for instance, that the underemphasized data are equally coherent in suggesting that the sleep stages have no particular function. This possibility is added parenthetically to the end of chapter 4, but is hardly touched upon subsequently. Furthermore, the studies showing that 'behavioural and psychological' effects of REM deprivation in humans are minimal and that, indeed, REM loss might even be beneficial sometimes, are countered by admitting that 'the evidence is not so far entirely persuasive', but that 'there are nonetheless suggestions...of interesting effects of D-deprivation'. These turn out to be based on 'hard to quantify' findings and on impressions of subjects' behaviour after REM loss, impressions which were unable to be supported by any significant differences.

By the end of the book, one feels that perhaps too many interdependent hypotheses have been created. The role of catecholamines in waking behaviour needs to be established and it is probably unfair to argue that the theory 'explains' the cyclical nature of the sleep profile by maintaining that 'the length of time occupied by the S- and D-periods and by the cycle is roughly the amount of time that *might* be involved in macromolecule synthesis and in structural change'.

In fairness to the author, an immense effort is made throughout the book to draw attention to the speculative nature of the interpretations and opinions. The present reviewer, however, would feel happier if some of the caveats made in the earlier chapters were present in the summary. In particular, the suggestions that possibly all of sleep, despite the different EEG patterns during it, has a similar function and that the basic rest–activity cycle may have independent origins. Such a conclusion probably reflects an underlying feeling that this book will convince only those who have a wide knowledge of sleep research findings, but who hold no viewpoint as to the function, or lack of it, of the various sleep stages. There cannot be many who fit such a description.

M. HERBERT

A Physiological Basis for Personality Traits: a New Theory of Personality. By D. Lester. Springfield, Ill.: Thomas. 1974. Pp. viii + 130. $9.75.

This book presents a theory of personality which postulates that the predominance of one of the two divisions of the autonomic system determines the behaviour of the individual concerned; P-types show parasympathetic predominance and S-types show sympathetic predominance. S-types are themselves subdivided into adrenalin and noradrenalin reaction types. Lester gives a brief and completely inadequate discussion of autonomic nervous system activity (he does not even mention such vitally important concepts as the 'visceral brain'), and then begins to interpret well-established personality theories in terms of his conception. He begins with Petrie's reducers and augmenters, concluding that 'there is some evidence that...reducing–augmenting is related to the balance between the sympathetic and parasympathetic systems'. This is rather a strong conclusion, seeing that the only evidence he adduces is some case studies of the effects of chlorpromazine, which apparently 'makes people more augmenting'. He next comes to introversion–extraversion; 'although there is little evidence to link the dimension of extraversion–introversion to the balance of the autonomic nervous system, such a link seems reasonable'. Not to this reviewer.

Lester goes on to repressors and sensitizers; he considers that sensitizers are P-type individuals whereas repressors are S-type individuals. But if introversion–extraversion is related to the P-type and S-type dimension, how is it that the repression–sensitization dimension does not correlate with E? The evidence suggests that sensitization is simply another name for neuroticism. Field dependence is next; apparently field-dependent people are S-types. But what of the evidence that there is no such dimension as field dependence at all, and that such small correlations as exist between different tests of it are entirely due to IQ? Next, in a rather confusing chapter, we are told that brain-damaged people are S-types, as are blue-eyed people; brown-eyed people are P-types. Similarly, levellers are S-types, sharpeners P-types. Sensation-seekers are S-types, as are psychopaths and philobats. Anger-in responders are S-types, anger-out responders P-types. Sheldon has a special chapter reserved for himself; ectomorphs are P-types, endomorphs and mesomorphs are S-types, with the former belonging to the adrenalin subtype, the latter the noradrenalin subtype.

Abnormal types are not forgotten. Alcoholics are P-types. Manic-depressives are S-type individuals, subtype A, while paranoids are S-types, subtype N. Schizophrenic are P-types, pretending to be S-types. Lester comments on the pervasiveness of the theory, and its ability 'to generate an immense number of testable hypotheses'. I am worried about this pervasiveness, and I doubt the testability of the hypotheses. A book that lays so much stress on autonomic functions without mentioning Lacey's specificity theory is clearly not taking its theoretical duties very seriously. The book bubbles over with ideas, analogies, suggestions and possibilities, but it is very short on facts providing the link between all these theories and properly measured and carefully investigated autonomic functioning. Lester clearly has no idea of the difficulties attending such a link-up. Perhaps a reading of the Venables and Martin *Manual of Psychophysiological Methods* might have brought home to Lester the enormity of the task, and the irrelevance of most of his alleged 'proofs'. This book clearly will not do. H. J. EYSENCK

Advances in Educational Psychology. By MIA KELLMER PRINGLE and VED P. VARMA. London: University of London Press. 1974. Pp. 292. £4.70.

In general, too many books are published each year under such titles as 'Recent Advances in . . ., Volume X. Particularly is this true when they do not represent a sifting of the best papers from a variety of journals over a period of, say, five years within a well-defined field, but rather a somewhat miscellaneous collection of contributions from a distinguished list of scholars, each of whom has been asked to write about what he desires in whatever way he chooses. The present volume is designed as a tribute to the work of Professor Wall, and while one is bound to admire Wall's distinguished services to education in so many leading capacities over many years, the publication itself might well have proved trivial or scrappy or both. Indeed, these qualities are by no means absent: Hotyat's chapter on learning is humdrum and superficial; Tizard's chapter on the Isle of Wight study merely summarizes a (justly) well-known report and the same is true of Tanner's chapter. Leonard's chapter on mobility training for the blind is rather specialized, as also is Osterrieth's account of the Draw-a-Man Test at adolescence. Lovell's chapter on intellectual aspects of adolescence adds little (beyond a couple of references) to what he and others have said more cogently and fully elsewhere. Bloom's chapter on affective consequences of school achievement is stimulating but falls far short of an adequate review, and Husen's exciting comments on futurologic studies have little to do with the rest of the volume.

Despite all this, the book turns out to be quite valuable. This is because the central chapters are concentrated on a well-defined area: the needs of exceptional children. It contains excellent chapters by Chazan on the maladjusted, by Gulliford on remedial education, by Gardner on brain damage and its treatment, and by Robb on the education of the gifted. (There is also a contribution by Sir Cyril, but this contains nothing new.) To these one might add Hemming's thoughtful analysis of emotional and social problems of adolescence (Mays' chapter on the same theme is much less weighty). All of these are well documented, thoughtful and even searching surveys of their respective fields. Chazan's chapter is particularly good. There is no general introduction to educational psychology for British students and practitioners in the clinical field. Half a shortish book is clearly not an adequate substitute for a proper introductory manual. But it is still a useful work of reference, and as such I must commend it. E. A. LUNZER

Paternal Deprivation. By H. B. Biller. Massachusetts: Heath. 1974. Pp. xi+226. £6.85.

In dealing with his subject, the author presents a critical account of the literature concerning the effects of father absence or inadequate fathering, and also makes frequent references to his own extensive researches.

The effects of paternal deprivation on both boys and girls are discussed, although relatively little work has as yet been done on the father–daughter relationship, and the effects on cognitive and academic functioning are emphasized as well as the more traditional areas of sex-role development and personal and social adjustment. An interesting trend is the focus on the father–infant relationship as one which may be of much greater importance than was previously acknowledged.

One of the main points which emerges is that many researchers have treated father absence in an overly simplistic fashion. It must now be clear that not only is a multidimensional approach essential, but that there is a need for some direct observation of interactions within the family to be built into experimental designs. The section of the book which deals with the way in which variations in mothering may be related to individual differences in the paternally deprived boys behaviour gives an illustration of the value of looking more widely afield. It would also be true to say that too little attention has been paid to those individuals who have succeeded in making a good adjustment. Information on why these children differ from those who are less well-adjusted may be of great importance, not least to the clinician who is attempting to modify the environment.

The theme of treatment and the implications of the research which have been cited are taken up in the final chapter of the book. The author deals firstly with current and potential modes of treating and preventing problems associated with paternal deprivation, and then puts forward a number of guidelines for the effective fathering of the young child. Among the issues raised in relation to treatment, is the importance of the participation of the father, where present, in any therapeutic procedures – a concept to which we too often pay only lip-service. He also argues convincingly for changing the feminized atmosphere of the classroom, particularly where younger children are concerned. An earlier chapter deals in some depth with the difficulties encountered by paternally deprived boys who are already insecure in their basic sex role orientations and who are faced by yet more female authority figures, and an emphasis on behaviour incompatible with a masculine role. While by no means all of those children who are paternally deprived are seriously at risk, it is not only in the United States that the problems encountered by fatherless or inadequately fathered families are drawing themselves to our attention. Approaches which tackle the problem before the crisis point at which the child is referred to the clinic are among those which cannot be ignored.

JEAN ROBERTSON

Families and Family Therapy. By Salvador Minuchin. London: Tavistock Publications. 1974. Pp. viii+268. £4.35.

This is a major contribution to the literature on clinical practice. It proposes a group of methods for modifying relationships in the family, and examines them with reference to issues about which every psychotherapist must take positions; issues like the meaning of symptoms, the connexions between psychic life and behaviour, the reasons for behavioural change, and the most expeditious ways of achieving it. The author's positions on these issues are integrated in a system of principles which he describes with exceptional clarity, and with so many examples that almost any reader will be able to picture in detail what he does and why he does it.

Seizing the bull by the horns, Minuchin states immediately that what might seem like individual pathology is defined by interactions among family members. He then describes how limits on these interactions are set by extra-familial events like the father's loss of a job or relocation of the family. Finally, he compares the implications of his position with that of the individually oriented psychotherapist, citing illustrative events in his clinic for diagnosis and psychotherapy.

Dr Minuchin is somewhat casual about tracing the origins of his theory. It is obvious that his diagrams of relationships, subsystems, boundaries, conflicts and coalitions owe much to G. H. Mead,

H. S. Sullivan, K. Lewin, and various systems theorists. These traditions are acknowledged only by reference to Bateson and Goffman, two writers who have drawn on the work of the masters in their work on psychopathology.

Dr Minuchin is at his best when writing about the therapeutic process. He proposes that the psychotherapist, like the anthropologist, can understand people's problems only by joining the family as a functioning member. As a result of his participation in ongoing activities, he develops a tentative picture of what is going on, identifies areas of possible change, creates imbalances in patterns of interaction and, finally, introduces and reinforces alternatives.

Much of the book is devoted to specific methods, the effects of which are diagrammed by a helpful code very reminiscent of Lewin's. Continually alerting his reader to the systematic aspects of the family's functioning, Dr Minuchin describes, and illustrates, an impressive range of therapeutic manoeuvres. Among the manoeuvres are the breaking up of undesirable coalitions, the changing of the distribution of power, supporting excluded members, creating distance when necessary, bringing members closer together, and clarifying boundaries that define the appropriateness and inappropriateness of different kinds of behaviour.

The simplest English is used throughout the book. It is refreshing to read a volume that differs so much from the jargon-ridden treatises prepared by so many clinicians. The clarity may be illustrated by the section on a particular problem: one member of the family dominates the discussion, making it difficult for others to disagree, to raise their own topics, or even to speak. To loosen this person's control, Dr Minuchin says that one might send him out of the room so he can watch the others from behind a one-way mirror, seat him at a distance from the rest so that it is harder for him to participate, or join others in actions that help to exclude him. The clinician might even attack the offending member, mimicking the ways in which he attacks the others.

The possible advantages and disadvantages of each action are carefully considered, and connexions frequently made with the presenting symptoms and the goals of therapy. In one case, for example, Dr Minuchin attributed a daughter's psychogenic vomiting to her being used as a scapegoat by her parents, who were in frequent conflict. One step on which he decided was to ask the family not to discuss the symptom. He had a number of objectives in setting up this barrier to communication: he intended that the family become less preoccupied with the official symptom; he hoped that this would force the parents to work on the difficulties in their marriage; he provided a support for the daughter that might help her to be more autonomous.

Two other features of the book deserve special mention: the section on pitfalls, and the transcripts of complete therapeutic sessions. Alert to the dangers of his methods, the author carefully considers a number of instances in which the therapists created too much stress in the families, were insensitive to the implications of their treatment for relationships that did not involve the official patients, or were not sufficiently alert to the developmental sequence of relationships. In each instance he discusses the resulting difficulties with reference to his theoretical principles, and proposes other methods that might have avoided the unfortunate consequences of the therapy.

Transcripts are provided of sessions with 'normal' American and Israeli families, and four families with disturbed members. The former highlight interactions between family and community, cultural differences and developmental sequences. The interviews with disturbed families highlight therapeutic goals and strategies by including the therapists' retrospective interpretations of events and their explanations of various manoeuvres. By including these chapters, Dr Minuchin joins Melanie Klein, Carl Rogers and a small coterie of others who have had the courage to record in detail the nature of their therapeutic activities.

The clarity with which the author describes where he stands will make it easy for readers with other viewpoints to formulate points of disagreement. Very debatable, for example, is his assumption that a transformation in the interpersonal patterns of a family activates internal changes within each member, which, in turn, elicit new patterns of social relationships that are self-reinforcing. This view of psychotherapy often leads Minuchin to ignore cognitive processes that would be central to the thinking of other practitioners. Anticipating their probable objections, particularly those of the individually oriented psychotherapist, he summarizes them and explains why he disagrees with them. His answers do not diminish this reviewer's doubts about the virtual disregard of internal experiences.

Two other questions are raised by the transcripts. All the patients seem very articulate, and one wonders how effective the therapies would have been if the families had included non-verbal members of the lowest parts of the working class or inarticulate schizophrenics. The relative poverty of the patients also suggests that the methods may be designed to provide effective, short-term therapy for people who cannot afford longer term, intensive methods. Some readers may disagree with the author about the probable effectiveness of the latter.

As stated earlier, the doubts as well as new ideas that are likely to come to the reader's mind bear vivid testimony to the clarity with which Dr Minuchin raises issues that must be considered by every psychotherapist, and the cogency of the arguments he advances in favour of his methods. This book should be read by every clinician.

D. R. MILLER

Preventive Educational Intervention for Mental Health. By S. RICHARD SAUBER. Cambridge, Mass.: Ballinger. 1973. Pp. xvii + 182. £4.80.

The message of this book is that we can do much to help ourselves and each other to cope with the stresses, strains and crises of life and in so doing we not only alleviate the burden on the official mental health service but may actually prevent, to a considerable degree, the incidence of mental illness.

There is a lack of consensus regarding the definition of mental health and mental illness. Sauber maintains that mental illness is social, not only in definition but also in origin. Evidence for this is drawn from statistics showing the varying rates of 'healthy' and 'unhealthy' individuals in different populations. This suggests that the criterion used for judging mental health is not the same for all societies, or for all sections of one society.

Historically, the mental health effort has been concerned with the mentally ill – with cure rather than prevention. This emphasis is a consequence of the predominance of a medical model of mental health, based on the notions of disease, illness and treatment. The author argues for a refocusing on social factors and on the prevention of mental illness through the promotion of positive mental health. There are already efforts to establish this ideal, seen in the total approach of the communes and hippie culture, and, more specifically, in encounter groups used in psychotherapy, and counselling in education, business, etc.

Sauber considers that the promotion of mental health necessitates a comprehensive programme involving the community as a whole. Mental health professionals using present methods are unable to meet the needs of the population. If the problem is largely social then the latent resources available in the community should be harnessed to help solve it – these are the semi-professionals and lay-people from whom individuals seek advice and comfort when in difficulty.

Sauber points out that people are generally uninformed about mental health, and may have a distorted view of it due to the sensational aspects exploited by the mass media. He might also have mentioned the problems created by fear and superstition about mental illness which accompanies this lack of knowledge. He argues that what is needed are simple facts and practical methods for dealing with mental health problems. He then presents practical educational programmes aimed at community service personnel (teachers, clergy, police, doctors, welfare officers), volunteers, and the public in general. The care at present given by community service personnel is often uncoordinated and thus fails to attend to the overall needs of the individual. Inadequate communications frequently result in duplication, fragmentation and discontinuity of patient care. Certainly, any programme aimed at community service personnel must emphasize the need for better integration of the existing services, but Sauber seems unduly optimistic about the willingness of personnel to accept modifications and extensions to their existing role, and of the capability of many of them to accommodate such demands. The recruitment of non-professional volunteers is seen as a particularly important aspect of a more general community service. Sauber rightly argues that a volunteer may be in a position to do what a professional cannot, such as establish a peer relationship, be involved in the patient's everyday routine, empathize with his life style. However, the development of this potential source of manpower may be difficult to implement as it is commonly assumed that only highly specialized and qualified professionals are capable of dealing with mental problems. Professionals themselves may also resist what will inevitably be viewed by many as a threat to their occupational esteem and integrity. Programmes for the public should aim at continuous adult education. The idea of the community school provides adults, as well as children, with the opportunity of pursuing academic, recreational, social service, and civic activity. Within the framework of a more closely knit and supportive community, it might be possible to gear courses more closely to social problems such as drug abuse, marital separation and suicide attempts, and to give information and advice on how to cope with these. Possession of suitable information should lead to a more sympathetic, if not more capable, response to the problem of mental health.

Chapter 7 deals with the important issue of evaluation. Sauber specifies questions to consider when evaluating each stage of a programme of intervention. He admits that the evaluative research in the field of community mental health is at a very primitive stage and points out the difficulty of persuading the community to support a project whose value and effectiveness has not yet been demonstrated.

While accepting the general thesis that mental health should be brought more to the centre of community life, the possible dangers of allowing non-skilled people too much autonomy should not be under-emphasized. Perhaps more stress should be laid upon creating a structured, organizational framework which would involve highly qualified consultative personnel at crucial points both to advise the semi-professional and volunteer force and, simultaneously, to monitor their contributions to the overall effort of mental health care.

Despite these reservations, the author has produced a useful introduction to this field, and the book will probably find its greatest use as a practical guide for those involved in courses on community welfare.

MAUREEN V. COX

Logical Abilities in Children. Volume 1. By D. N. OSHERSON. New York: Wiley. 1974. Pp. x+162. £5.30.

Children's thinking is said by the Geneva school to develop through a succession of stages defined by the appearance of logical structures. Osherson's monograph – and it is quite definitely a specialist monograph – addresses itself to an important and neglected problem raised by their approach. This is the relation between the axioms of a structure and its theorems.

Piaget has on several occasions defined his logical structures informally by listing their 'properties'. The task of axiomatization has fallen largely to J. B. Grize, who has produced axiomatizations of both the major structures, the grouping structure and the INRC group. Given that the Geneva school have embarked on this reduction of the multitudinous, in fact infinitely many, 'laws of thought' (theorems) involved in the structures to certain 'axioms of thought', what role do these axioms play in the child's thinking?

Osherson sets out to investigate one possible conception of this role. Whether this happens to be Piaget's conception or that of someone else in the Geneva school is, as Osherson points out, difficult to know; but that is ultimately unimportant. His idea is that given the child knows all the axioms necessary and sufficient for a theorem, he knows the theorem. He reports two experiments relating to this conception, as applied to knowledge of the grouping structure in young children, and concludes, not surprisingly, it is in error; because if it were true we would be much better mathematicians than we are. The adult who has not studied mathematics is unlikely to divine either the existence or Pythagoras' proof of the existence of irrational numbers, although he probably knows the axioms of arithmetic.

It is not so much the failure of Osherson's experiments to support his hypothesis, but rather the partial success of the hypothesis that requires explanation. Very likely the theorems he chose were relatively easy to derive from their axioms and so unrepresentative of theorems in general. Osherson himself tries to meet this general point, but his approach to the problem is disappointing, being through the manipulation of empirically derived task-specific weighting coefficients, rather than any theoretical analysis of theorem difficulty. What is more interesting from the Genevan point of view is his comparison of the same set of grouping structure axioms and theorems realized in two different embodiments: length relations and class inclusion. While Osherson's 'instant deduction' theory works quite well for length, the exactly parallel connections between axioms and theorems in the domain of class inclusion are apparently much less likely to be made by the child. It is difficult to even hazard a guess as to the reasons for this, as Osherson tells us too little about the detailed results from the class inclusion experiment. Rather than attempting any constructive development of the Geneva theory he wishes, and indeed invites, us to draw the conclusion that the failure of his own hypothesis is an object lesson in the sterility of the Geneva approach. But he knows they will say it was his hypothesis, not theirs.

P. E. LANGFORD

The Dyslexic Child. By T. R. Miles. London: Priory Press. 1975. Pp. 140. £3.50.

It is possible that Professor Miles' main purpose in writing this book was to reassure anxious parents whose reasonably intelligent children are unable to learn to read and spell efficiently. It is perhaps unfortunate that the cases he describes are mainly of older children who are able to read but cannot spell, since the plight of those who cannot even read is greater. Moreover, as he points out, it is extremely important that they should be detected as young as possible.

The book may indeed be helpful to parents in reassuring them that there really exists a definite constitutional disability of dyslexia which can and should be taken seriously and afforded suitable and adequate remedial teaching. However, it scarcely elucidates the nature of the disability. Thus it is not clear whether Professor Miles considers that there is a basic initial deficiency in the capacity to associate printed letters with the appropriate sounds; and if so, what visual, auditory-linguistic and/or conceptual processes are defective. Or does he believe that the main deficiency lies in the perception and remembering of the sequential order of letters in words; associated with reversals and with 'directional confusion' in identifying the right and left sides of the body? It is interesting to learn that dyslexic children are frequently handicapped in arithmetic because they do not remember digit sequences correctly. Nevertheless, insight into all these difficulties would be greater had they been more extensively and systematically tested and assessed, the results of testing related to age norms for normal readers, and the frequencies of the deficiencies estimated.

Though the causes of the disability are discussed, little can be said about these with any certainty, except that there is a greater probability of dyslexia when other members of the family are affected. But the book may well be useful for parents and teachers in giving a simple outline of the principal characteristics of dyslexia. They may also find it helpful to consider the points listed in Appendix II on how to recognize a dyslexic child; though possibly those listed for children of 8½ years and over are unnecessarily alarmist. Such children scarcely require special remedial teaching unless they read very badly and their spelling is grossly inaccurate. But unfortunately the book does not contribute greatly to that understanding of the nature of dyslexia which is necessary to dispel once and for all the illusion of many educationists (including the members of the Bullock Committee) that dyslexia need not and cannot be discriminated from other forms of reading backwardness.

M. D. VERNON

Experimental Psychology: an Introduction for Biologists. By Malcolm Jeeves. (The Institute of Biology's Studies in Biology no. 47.) London: Arnold. 1974. Pp. ii + 60. £1.70; paper, 85p.

Anyone who interviews biology sixth-formers for admission to a university will know of their great interest in psychology as an academic discipline. Since relatively few biology teachers will have much direct experience of psychology at this level there is an obvious need for an introductory text, and what better place for it than in this Institute of Biology sponsored series. Many of the books in the series have dealt successfully with rather specialized topics at an introductory level, but there are evident difficulties (as Professor Jeeves hints in his preface) in compressing a fairly extensive branch of biology into a 60-page booklet. Perception, Conditioning and Learning, Thought and Language (all chapter headings in this book) are after all as least as large in scope as the whole subject of some booklets in the series.

Professor Jeeves' task then is a pretty exacting one: to give some idea of the whole field of experimental psychology and at the same time of its methods; to arouse interest and to satisfy curiosity. In the circumstances he has done fairly well, at the cost of leaving a lot of loose ends and untidy joins. For example, in chapter 2 on Sensory Perception the links between the sections on cortical responses to visual movement, the responses of the newborn and the effects of sensory deprivation in early life are weak, so that one is left without a clear memory of what the subject of the chapter really was. The same fault runs through the book as the author struggles to describe one type of experiment adequately before he skates rapidly off to another. One way of coping with the problem might have been to concentrate information about particular experiments in tables and figures, leaving their wider significance to the text. This is after all the technique used in some of the highly successful *Scientific American* articles which make up part of the further reading list. In fact, the legends to the figures are the least satisfactory part of the book, as few of them are self-sufficient. The inclusion of an information measure of distortion in three figures sets in train a less than adequate account of elementary information theory in the text, and a stimulus-response matrix burdens the text with further explanation (which seems to have a mistake in it anyway). The experimental procedures used in investigating reversal and non-reversal shifts in discrimination learning are not clear to a naive reader after studying both figure and text.

These rather petulant criticisms stem from disappointment that a useful book with well-chosen contents should somehow prove so unsatisfying to read. All the figures and the descriptions of experiments are documented, and more information can be found by anyone with access to a good library, but not every sixth-former will be able to do this. When all this has been said, however, any biologist, from sixth-former on, will find a lot to interest him in this book, at the cost of a small amount of irritation.

J. SUDD

The Psychology of Art. By L. S. VYGOTSKY. Introduction by A. N. LEONTIEV, Commentary by V. V. IVANOV. Cambridge, Mass. and London: M.I.T. Press. 1974. Pp. xiv + 305. £2.00.

At least one leading Soviet psychologist has voiced the opinion that the dominant influence in contemporary Soviet psychological thinking is that not of Pavlov but of Vygotsky. Lev Semenovich Vygotsky died in 1934, at the age of 38. Until the publication in 1962 of an English translation of *Thought and Language* (a posthumous work even in its original Russian) Vygotsky was known 'in the West' mainly for the 'Vygotsky blocks', a device for the study of conceptual thinking deriving ultimately from the work of the Würzburg school during the first decade of the century. The present book, also unpublished in the author's lifetime, makes reference to that work, and, at greater length, to Wundt's views on emotion and imagination. Thus it can be seen to span the ages.

Leontiev, in his Introduction, suggests that following the completion of the book (in 1922) Vygotsky's 'internal motivations' led him into other areas; these include the work on language for which Vygotsky is now best known. That being so, it may come as less of a surprise to the reader than might otherwise have been the case to discover that in the present book art is virtually equated with literature. References to painting and to music are seldom more than tangential, and one cannot help feeling that the special case represented by literature influenced Vygotsky more than he himself might perhaps have been willing to admit. Thus Leontiev points out that 'the material [of literature] has meaning, is *relevant* to the content embodied in it'. Again, Vygotsky himself, in a discussion of Krylov's *Fables*, writes (p. 136): 'finally we give a psychological analysis of our reaction to the...fable, study the general mechanism of man's psyche set in motion by the fable, and determine the action man takes as a result of reading a fable.'

The greater part of Vygotsky's exposition is in fact based on Krylov's *Fables*; although *Hamlet* is also extensively quoted, an intimate knowledge of Russian language and literature is really necessary if one is to appreciate the author's argument to the full. Much the same applies to his discussion of views of the nature of art current in Russia at the time he was writing. Three chapters leading into Vygotsky's statement of his own position deal with 'Art as Perception', based mainly on the writings of the linguist A. A. Potebnia; 'Art as Technique' – essentially the 'formalist' position, as formulated by V. B. Shklovskii; and 'Art and Psychoanalysis', with perhaps more emphasis on Otto Rank than on Freud.

Vygotsky's own theory is itself summed up in a chapter heading: 'Art as a Catharsis'. Vygotsky's use of the term 'catharsis' differs from Aristotle's, and from Freud's. To quote Leontiev once again: 'To Vygotsky, catharsis is...the resolution of a certain, merely personal conflict, the revelation of a higher, more general, human truth in the phenomena of life.' Vygotsky himself offers other definitions: I have searched in vain for one sufficiently succinct and explicit to be quotable out of its context. It may be noted, however, that he lays much emphasis on the role, in catharsis, of form as against content; indeed, he goes so far as to say (p. 215): 'the artist achieves his effect by means of the form, which destroys the content'.

This is a difficult book, and occasional obscurities in the anonymous translation 'by Scripta Technica, Inc.' add to its difficulty. Nevertheless, one gets the impression of a work of a calibre equal to that of *Thought and Language*. Perhaps an explication, rather than a direct translation, was called for.

B. SEMEONOFF

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Printed in Great Britain

OTHER PUBLICATIONS RECEIVED

(Inclusion in this list does not necessarily preclude later review)

Bion, W. R. *Attention and Interpretation.* London: Tavistock. 1975. Pp. 136. 85p.

Bogooh, S. (ed.). *Biological Diagnosis of Brain Disorders.* Chichester: Wiley. 1974. Pp. xii + 392, £12.15.

Brown, G., Cherrington, D. H. & Cohen, L. *Experiments in the Social Sciences.* London: Harper & Row. 1975. Pp. 256. £2.95.

Buros, O. K. (ed.). *Tests in Print II.* Highland Park, N.J.: Gryphon Press. 1974. Pp. xxxix + 1107.

Coan, R. W. *The Optimal Personality.* London: Routledge & Kegan Paul. 1974. Pp. x + 242. £4.75.

Corman, L. *L'Interpretation Dynamique en Psychologie.* Paris: Presses Universitaires de France. 1974. Pp. 120.

Cull, J. G. & Hardy, R. E. *Behavior Modification in Rehabilitation Settings.* Springfield, Ill.: Thomas. 1974. Pp. xvii + 251. $12.00.

Davis, A. E., Dinitz, S. & Pasamanick, B. *Schizophrenics in the New Custodial Community.* Columbus, Ohio: Ohio State University Press. 1974. Pp. xxviii + 214.

Eldridge, J. E. T. & Crombie, A. D. *A Sociology of Organizations.* London: Allen & Unwin. 1974. Pp. 218. £4.25.

Foley, V. D. *An Introduction to Family Therapy.* London: Grune & Stratton. 1974. Pp. xiv + 207. £5.05.

Fontana, V. J. *Somewhere a Child is Crying.* London: Collier–Macmillan. 1975. Pp. xx + 268. £3.75.

Freud, A. *Introduction to Psychoanalysis.* London: Hogarth Press. 1975. Pp. xx + 200. £3.75.

Fried, P. A. *Readings in Perception: Principle and Practice.* London: D. C. Heath. 1974. Pp. xii + 548. $6.95.

Friedman, M. & Rosenman, R. H. *Type A Behavior and Your Heart.* London: Wildwood House. 1974. Pp. x + 276. £4.95; paper, £2.75.

Goldberger, L. & Rosen, V. (eds.). *Psychoanalysis and Contemporary Science.* Volume 3. New York: International Universities Press. 1974. Pp. xiii + 557. $15.00.

Gregson, R. A. M. *Psychometrics of Similarity.* New York: Academic Press. 1975. Pp. x + 262. £10.30.

Haim, A. *Adolescent Suicide.* London: Tavistock. 1975. Pp. xiii + 310. £6.00.

Herriot, P., Green, J. M. & McConkey, R. *Organization and Memory.* London: Methuen. 1973. Pp. x + 117. £1.50.

Hollos, M. *Growing up in Flathill.* Oslo: Universitetsforlaget. 1974. Pp. 166. $10.00.

Hudgens, R. W. *Psychiatric Disorders in Adolescents.* Baltimore: Williams & Wilkins. 1974. Pp. xii + 234. £7.00.

Inhelder, B., Sinclair, H. & Bovet, M. *Learning and the Development of Cognition.* London: Routledge & Kegan Paul. 1975. Pp. xiv + 308. £4.95.

Kay, W. *Moral Education.* London: Allen & Unwin. 1975. Pp. 383. £4.95.

Kazdin, A. E. *Behavior Modification in Applied Settings.* Homewood, Ill.: Dorsey Press. 1974. Pp. xii + 292. $5.50.

Koffka, K. *Principles of Gestalt Psychology.* New York: Harcourt, Brace & World. 1935 (in 1963 by E. Koffka). Pp. xi + 720. $3.95.

Krishna, G. *Higher Consciousness*. New York: Julian Press. 1974. Pp. 198. $7.95.

Krishna, G. *Kundalini*. London: Robinson & Watkins. 1971. Pp. 252. £2.25.

Lambert, R. *The Chance of a Lifetime*. London: Weidenfeld & Nicolson. 1975. Pp. 423. £7.00.

Lee, W. *Experimental Design and Analysis*. San Francisco: Freeman. 1975. Pp. 353. $15.00.

Lee, W. *Workbook for Experimental Design and Analysis*. San Francisco: Freeman. 1975. Pp. 116. £1.60.

Lesowitz, R. I. *Rules for Raising Kids*. Springfield, Ill.: Thomas. 1974. Pp. ix + 186. $6.95.

Levelt, W. J. M. *Formal Grammars in Linguistics and Psycholinguistics*. Volume I. The Hague: Mouton. 1974. Pp. x + 143. D.fl. 16.00.

Levelt, W. J. M. *Formal Grammars in Linguistics and Psycholinguistics*. Volume II. The Hague: Mouton. 1974. Pp. viii + 194. D.fl. 26.00.

Levelt, W. J. M. *Formal Grammars in Linguistics and Psycholinguistics*. Volume III. The Hague: Mouton. 1974. Pp. viii + 206. D.fl. 32.

Lindauer, M. S. *The Psychological Study of Literature*. Chicago: Nelson-Hall. 1974. Pp. xiii + 254. $11.00.

McFarland, D. J. *Motivational Control Systems Analysis*. London: Academic Press. 1975. Pp. xii + 523. £11.00.

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CONTENTS

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SOMMAIRE

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R. VANFRAECHEM et M. C. HENNUYEZ – Étude comparative des paramètres psychologiques et physiologiques de deux groupes d'étudiantes en éducation physique et d'étudiantes en philosophie.

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JULIUS WISHNER – Portrait, notice biographique et bibliographique.

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SOMMAIRE

C. DAURAT-MELJAC et M. VERBA-RAD – Evolution de l'utilisation du nombre dans la description d'une collection chez les enfants de 4 à 7 ans.

F. AMIEL-LEBIGRE, R. OLIVIER-MARTIN, E. BUCHSENSCHUTZ, J. HASSAN et J. LACASSIN – Essai de quantification de la symptomatologie en épidémiologie psychiatrique.

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CONTENTS

BRANTHWAITE, ALAN
Subjective value of information *page* 275

FREIDES, DAVID
Information complexity and cross-modal functions 283

GALE, ANTHONY, SPRATT, GRAHAM, CHRISTIE, BRUCE AND SMALLBONE, ADRIAN
Stimulus complexity, EEG abundance gradients, and detection efficiency in a visual recognition task 289

LALLJEE, MANSUR AND COOK, MARK
Anxiety and ritualized speech 299

GAULD, ALAN
A note on inappropriate constancy-scaling and the Müller–Lyer illusion 307

GELLATLY, ANGUS R. H. AND GREGG, VERNON H.
The effects of negative relatedness upon word–picture and word–word comparisons and subsequent recall 311

GODDEN, D. R. AND BADDELEY, A. D.
Context-dependent memory in two natural environments: on land and underwater 325

RICHARDSON, JOHN T. E.
Imagery and deep structure in the recall of English nominalizations 333

GUR, RUBEN C. AND HILGARD, ERNEST R.
Visual imagery and the discrimination of differences between altered pictures simultaneously and successively presented 341

WALKER, PETER
The subliminal perception of movement and the 'suppression' in binocular rivalry 347

BARBER, PAUL J. AND RUSHTON, J. PHILIPPE
Experimenter bias and subliminal perception 357

KRUGLANSKI, ARIE W.
Context, meaning and the validity of results in psychological research 373

BOYLE, D. G.
Research note: a new technique for studying phenomenal causality 383

BOOK REVIEWS 387

OTHER PUBLICATIONS RECEIVED 405

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RECOGNIZING FACES

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Following a review of the stimulus and subject factors which have been found to affect recognition of faces, the question of whether this process can be considered a special one is dealt with. Evidence from studies involving the development of face recognition, the recognition of inverted faces, and the clinical condition prosopagnosia is considered, and in each case found to be inadequate for the unequivocal conclusion that the processes underlying face recognition are qualitatively different from those employed in recognizing other pictorial material.

Recognition memory involves the identification of some previously experienced configuration or event. In recent years interest has shifted from being primarily concerned with verbal memory to include consideration of memory for pictorial material.

The question which seems to have provoked most comment concerns how good is our ability to recognize large numbers of pictures after just one brief exposure (e.g. Nickerson, 1965). Estimates of the recognition of hundreds of complex pictures (advertisements, etc.) in excess of 90 per cent accuracy are quite common, which seems much better than our ability to recognize a three-digit number shortly after seeing it (see Goldstein & Chance, 1974, for a critical review).

Pictures, though, are usually unique and are therefore less subject to the proactive and retroactive interference which reduces recognition of other stimulus materials such as digits. More significantly, as Goldstein & Chance (1971) point out, pictorial material may also allow dual coding, for the pictures used in the investigations quoted were quite heterogeneous, enabling them to be easily coded and remembered in a verbal as well as a visual form. 'In other words, what was stored in memory may not have been details of a particular stimulus but simply a general concept' (Goldstein & Chance, 1971, p. 237).

With the above consideration in mind, it would seem worthwhile to test pictorial recognition by employing fairly homogeneous material, thereby reducing the risk of verbal mediation. Goldstein & Chance (1971) adopted such a strategy and compared recognition memory for three classes of pictorial stimuli – faces, ink blots, and snow crystal photographs. They observed that faces were recognized best, but, of course, it would be fruitless to pursue between-class comparisons, since it is impossible to equate the three types of material for familiarity and difficulty of discrimination.

Yin (1969) compared the ease of recognizing pictures of faces with pictures of houses, aeroplanes and schematized men in motion. His results indicate that far fewer errors occur with faces compared with the other homogeneous classes of stimuli. Likewise, Scapinello & Yarmey (1970) found that faces were slightly better remembered than pictures of dogs or buildings. These findings are interesting because in both studies the comparison was between faces and familiar objects rather than the unfamiliar snowflake and inkblot patterns used by Goldstein & Chance (1971).

Deregowski *et al.* (1973), however, using cups and faces as stimuli, were unable to find a difference in their memorability with samples of either African or European subjects. This result does not necessarily show that memory for faces is not superior to that for other classes of homogeneous pictorial material, but it does serve to illustrate that the selection of exemplars of a class can determine the apparent ease of recognition.

Clearly, faces constitute a psychologically special class of stimuli. They are extremely important to us in our everyday lives; they are frequently 'identified' in the sorts of unstructured patterns provided by clouds and flames; they can convey information about an individual's emotional state (Ekman *et al.*, 1972); and they are a potential source of information from which we make attributions about other people (Hochberg & Galper, 1974). It is not surprising, then, that there has been a great deal of research concerned specifically with the memory for faces. Some of this work has been aimed at elucidating the proposition that the mechanisms underlying facial recognition are different from those involved in recognizing other classes of pictorial material (e.g. Konorski, 1967; Yin, 1969). This particular research line will be assessed later in the paper following a general review of the literature concerned with recognizing faces.

In the following sections various studies concerned with the recognition of faces will be classified and commented on. The review, however, will not be concerned with *recall* of faces. This aspect of facial memory has been studied with schematic faces (e.g. Smith and Nielsen, 1970; McKelvie, 1972), and with Photo-fit reconstructions of photographed faces (Ellis *et al.*, 1975), but as yet no really suitable instrument for monitoring recall of real faces is available.

STIMULUS FACTORS

Most studies have involved recognition memory for photographs of stimuli. Surprisingly, there appears to be no difference in recognition rates as to whether colour or black and white pictures are employed; nor does it matter whether the pose is frontal, three-quarter or full profile (Laughery *et al.*, 1971). As yet, no one has examined the relative effect on recognition of showing three-dimensional and two-dimensional faces, but it is unlikely that much difference would be found, unless perhaps the three-dimensional faces are moved in the expressive manner normally encountered when viewing a real face. Such movements might assist later recognition through additional verbal coding (e.g. 'the one who always smiles').

Feature saliency

Faces are complex patterns which comprise a number of sub-patterns which are not equally effective in assisting recognition. Howells (1938) observed that covering the lower half of a face during initial presentation was more deleterious to later recognition than covering the upper half of the face. Goldstein & Mackenberg (1966), however, testing school children, found the opposite result. Both of these results can be seen to be in accord with the introspective report made by the subjects of Laughery *et al.* (1971), for their subjects ranked the importance for recognition of various features in the order: eyes, nose, mouth, lips/chin, hair and ears. From analyses of the visual inspection strategies employed in looking at faces it is clear that the eyes receive most attention (Zusne, 1970). Similarly, young infants first concentrate their attention on the eyes of a face and only later fixate the mouth region. Whether these observations have any great bearing on the problem of face recognition, however, is debatable. Certain areas, namely the eye and mouth regions, are more mobile than others and so convey more information about an individual's mood which can assist us in comprehending what they are trying to communicate to us. Therefore we might pay more attention to, and learn to differentiate, those features more effectively, but it might be a mistake to examine a subject's performance when shown partial information since his behaviour may not be identical to that occurring when he is allowed to view the whole face (i.e. a Gestalt).

The 'saliency' of a particular feature or part of the face can be examined in other ways. There is convincing evidence, for example, that people prefer the left half of the face before them (i.e. actually the right half of the face). Gilbert & Bakan (1973) showed that when a face is split down the middle and each half is duplicated in reverse to make a whole 'face', subjects report that the 'face' composed of the two left halves is more like the original face than that made up of the two right halves. Such preference does not derive from any natural asymmetry in the expressiveness of the two halves of the face as some have suggested (e.g. McCurdy, 1949), since, when the 'original' is shown as a mirror reversal, the tendency remains for subjects to prefer the left–left composite. This is also true of Israeli subjects who normally read in a right–left direction, and so Gilbert & Bakan disclaim any explanation in terms of the Western left–right reading habit. Instead, they explain their results as being due to the features falling in the left visual field being processed by the right hemisphere, which, as we will see later, is strongly implicated as the dominant hemisphere for processing visual patterns. Their hypothesis, however, can only suffice in the unlikely event of fixation being maintained at some central point on the face. A more plausible explanation can be postulated based upon a model of asymmetry in cerebral attention proposed by Kinsbourne (1973). He argues that when either hemisphere is temporarily dominant (the left hemisphere during verbal activity, and the right during the analysis of visuo-spatial material), there is a tendency for contralaterally directed eye movements to occur (i.e. to the right when the left hemisphere is ascendant, and to the left when the right hemisphere is more active). Thus when looking at faces the right hemisphere is more active than the left hemisphere, and so there is a fixation bias towards the left half of the visual field that could well mediate the preferences reported by Gilbert & Bakan. Whether the left half of a face is more influential in determining later recognition has yet to be established, but the likelihood is that faces are usually too nearly symmetrical for any differential effect to matter very much.

Facial attractiveness

Although it is a fairly commonplace observation that some faces are easier to remember than others, and while this has also been observed in laboratory experiments on face recognition (e.g. Goldstein & Chance, 1971), surprisingly little research has been attempted to investigate what attributes of a face determine ease of recognition. Cross *et al.* (1971) examined the role of 'beauty' in recognition and found that the faces initially rated high in beauty were subsequently better recognized. Unfortunately, they did not control the amount of time initially spent looking at each face. This leaves the possibility that subjects merely spent a greater time viewing the more attractive faces, and so coded the information better than when looking at less attractive faces.

In an early study, Peters (1917) examined the relationship between the 'pleasantness' of faces and their memorability. Pleasantness is obviously not synonymous with beauty but there is probably enough of an overlap between the two concepts to make some comparisons between Peters' study and that of Cross *et al.* Unlike the latter experiment, Peters controlled the time for which subjects saw each face, obtaining a rating of pleasantness after the subjects had seen each face on a second occasion and had given their judgement as to whether they had seen it before or not. Pleasant faces were remembered best, but faces rated low in pleasantness were recognized next best, with faces of intermediate pleasantness being recognized worst.

More recently, Shepherd & Ellis (1973) investigated the ease with which girls' faces, which varied in attractiveness, were recognized. Attractiveness was measured by ratings given by a large group of subjects who were not employed in the memory task. They, like Peters, found a curvilinear relationship, but only when the faces had to be retained over a period of a month. Then the faces in the top third and bottom third of the attractiveness dimension were equally well recognized, and both were significantly better recognized than those from the middle third of the range. Shepherd & Ellis explain these data by suggesting that high and low attractive faces are more arousing that those of medium attractiveness, and that this produces stronger memory traces (cf. Kleinsmith & Kaplan, 1963).

Facial expression

Whilst much work in the past has concerned the perception of mood from photographs of faces, little has been done to determine the effect of mood-expression upon recognizability. Galper & Hochberg (1971) examined the performance of subjects who were shown either a smiling or an unsmiling picture of a face and then tested for recognition with the same face posed in the alternative expression. This condition led to poorer recognition scores in a paired comparison test conducted five days later than those obtained when the stimulus face was paired with a different face altogether. Unfortunately, Galper & Hochberg did not examine whether subjects were more accurate at recognizing faces initially seen in a smiling pose compared to an unsmiling pose. A difference might be expected if one assumes that smiling faces evoke more affect and would normally be rated more pleasant than unsmiling ones.

Transformation of faces

Change of expression is one form of stimulus transformation. Two other forms of transformation of faces have also received attention. These are (*a*) inversion and (*b*) photographic negative.

Inversion. When he presented and tested for recognition of stimuli in an inverted orientation, Yin (1969) found a general increase in errors compared with presentation in the normal upright orientation. This increase was almost fivefold for faces but far less for pictures of houses, aeroplanes and caricatures of men in motion. He also found line-drawings of faces subject to similar increases in error when inverted.

Hochberg & Galper (1967) had observed a similar drop in performance for inverted faces which was unaffected by presenting the faces in an upright position during the recognition test. Investigations by Scapinello & Yarmey (1970) and Yarmey (1971) have confirmed that recognition errors increase when faces are rotated through 180°. Differential performance on the recognition of upright and inverted faces appears to occur quite young, for Fagan (1972) found that 5–6-month-old infants could reliably distinguish a previously seen upright face from a novel one, but they were unable to do so when the faces were presented in an inverted orientation.

Bradshaw & Wallace (1971), however, demonstrated that subjects very quickly learn to discriminate the features of an inverted face in a visual search task. This suggests that, given enough practice, subjects could achieve much higher levels of recognition performance on inverted faces. As Rock (1974) has pointed out, inverting a face alters a number of spatial relationships with which we are extremely familiar. Such an alteration could upset any acquired scanning strategy for viewing faces. Noton & Stark (1971) showed that people employ a consistent pattern and order of eye-movements when looking at a particular picture. It seems likely also that people employ a consistent eye-movement pattern for looking at faces, which would be inappropriate for inverted faces. Smith & Nielsen (1970) found that subjects tended to use a top to bottom strategy for examining schematic faces which could be less efficient for looking at inverted faces. Alternatively, it might simply take time to acquire an efficient scanning pattern for any novel class of object (e.g. inverted faces), and that until sufficient experience with that class occurs the recognition process will be inefficient relative to that for familiar objects (e.g. upright faces).

Photographic negatives. Galper (1970) first examined the ability of subjects to recognize photographic negatives of faces. She found that compared with a positive–positive condition, both negative–negative and negative–positive led to poorer recognition – but they themselves did not produce significantly different mean error rates. Galper suggested that her results might be due to a difficulty in determining the expression of a face from a photographic negative. But there is no evidence to suggest that the expression of a face has much bearing on the memory processes underlying facial recognition. Phillips (1972) examined yet another possible explanation: he observed that a negative of a face reverses the whole range of brightness relationships found in black and white photographs of faces. Therefore he presented lith photographs (a process which renders dark greys black and light greys white) in both positive and negative forms, and found that lith negatives (which involve a simple black–white white–black transformation) were not as well recognized as lith positives of the same well-known faces. Phillips interpreted this finding as signifying that the difficulty in recognizing faces from negatives is not simply due to the reversal of grey tones.

As with Galper's study, Phillips' experiment did not allow for practice with the transformed faces. This would seem necessary before asserting (as Galper does) that a negative face contains the same amount of information as a positive one, since it seems likely that when subjects are confronted by a strange transformation of a previously well-learned set of brightness relationships, they will require time to learn to use the novel information. This argument, of course, is not confined to faces:

neither Galper nor Phillips looked at people's ability to recognize other pictorial scenes shown in photographic negative. It is conceivable that such a study would reveal an equally strong impairment in recognition memory.

SUBJECT FACTORS

Just as a number of stimulus factors in recognizing faces have attracted experimental attention, so various subject factors have been examined. In particular, the age, sex, personality and handedness of subjects have yielded interesting data.

Age

Most cognitive tasks reveal an improvement in performance throughout childhood and adolescence. It was surprising therefore to discover that Cross *et al.* (1971) were unable to find a significant improvement in correct recognition scores for adolescent faces among groups of 7-, 12-, 17-year-old and adult subjects. However, they did observe that the younger subjects made more false identifications (i.e. saying they had seen a face previously which had not actually occurred in the initial presentation array). When these false identification scores are combined with the correct recognition ones to form the statistical decision theory discrimination parameter d', then a clear developmental trend is evident (7-year-olds, $d' = 0{\cdot}61$; 12-year-olds, $d' = 0{\cdot}93$ 17-year-olds, $d' = 0{\cdot}99$; adults, $d' = 1{\cdot}95$).

Moreover, Ellis *et al.* (1973) tested 12- and 17-year-old subjects and found the latter to be significantly superior at recognizing faces of young adults on the basis of both correct identification and d' scores. The older adolescents, it might be said, are probably more familiar with faces of young adults and as such were at an advantage, but younger children are not better at recognizing faces drawn from their own age group which they might be expected to be more familiar with (Goldstein & Chance, 1964). This suggests that this particular explanation is less likely than one where intelligence or cognitive maturity is proposed as the important factor.

Sex

While there is no sex difference in the ability to recognize schematic faces (McKelvie, 1972), Howells (1938) found women's recognition scores of photographs of faces to be insignificantly superior to men's, and a later study by Witryol & Kaess (1957) indicated women's scores to be significantly better. Witryol & Kaess also observed that male subjects tend to recognize men's faces better than women's, whereas female subjects were significantly more often correct at identifying women's faces.

Goldstein & Chance (1971) also found women to be better than men at recognizing female faces (they did not employ male faces), but could find no such sex difference for recognition of inkblots and snowflake crystals. Cross *et al.* (1971), however, whilst finding no overall sex difference did observe that female subjects were significantly better at remembering women's faces. Their male subjects, however, had equally good recognition scores for faces of either sex. Likewise, Ellis *et al.* (1973) found that girls were significantly better than boys at recognizing women's faces, but while the girls were also found to be superior at recognizing men's faces this was not statistically significant. Both Cross *et al.* and Ellis *et al.* explain their sex of subject × sex of stimulus interaction by pointing out that women are exposed to a large number of female faces via cosmetic advertisements, etc. and that this experience might improve their discriminatory facilities for coding female but not male faces.

This explanation does not, of course, entirely account for Witryol & Kaess' findings. In addition, it is unable to account for Fagan's (1972) observation that 5–6-month-old girls appear to have a better memory for photographs of faces than their male peers. This finding does not seem to be contingent upon any greater perceptual maturity for infant girls compared with infant boys (Fagan, 1972) – indeed, it has generally been accepted that infant boys show *more* interest in visual patterns (Hutt, 1972). Then why should infant girls (and, possibly, adult women) show better retention for physiognomic patterns? It is possible that girl infants may have already learned to associate sounds (to which they are more responsive than boys, Hutt, 1972) with faces, and thereby have learned to differentiate faces more readily than have boys simply because they have attended to them more often. The issue, though, might be resolved in a less theoretically interesting way, for Fagan (1973) was unable to replicate the sex differences in memory for faces when he slightly modified his experimental technique. Clearly, some further investigation of the sex effect is required and an analysis of the methodological differences between Fagan's studies should be made.

Field dependency

Yet another explanation for adult sex differences in recognizing faces is in terms of the cognitive style known as field dependency. Witkin *et al.* (1962) suggested that field-dependent persons, being in need of support and guidance from others, are particularly attentive to facial characteristics. As a consequence, field-dependents 'tend to be better than relatively field-independent persons at recognizing people they have seen only briefly before' (p. 3).

Messick & Damarin (1964) made an empirical assessment of this assertion. First, though, they reported the results of an unpublished study by Crutchfield *et al.* (1958) that support the contention. This study involved the ability of air force officers to recognize photographs of people they had previously met. Success in this task was found to be positively associated with field dependence as measured by the rod-and-frame test.

Messick & Damarin themselves tested 50 subjects of whom 40 were males. These were presented with pictures of faces whose ages they were asked to assess. The ability to subsequently recognize the faces was found to correlate with errors on an embedded figures test which, it is claimed, is a measure of field dependence.

Interestingly, women have been found to be more field-dependent than men (Witkin *et al.*, 1962), and it is therefore not surprising that they have been found to be more able in facial recognition tasks. Unfortunately, the study by Messick & Damarin involved too few women to examine any sex effects.

Need for approval

Schill (1966) found that under a certain condition those with a high need for approval have better memories for faces presented in an age guessing (i.e. incidental) task. This condition involved the experimenter making either positive or negative verbal reinforcement for age guesses: when no reinforcement occurred, no difference between those high and low on need for approval was obtained. There has been no follow-up to this study, and so the relationship between need for approval and general facial recognition has not been fully explored.

Handedness

In a recent study by Gilbert (1973), the relationship between handedness and memory for male and female faces was examined. He found that, while strongly left- and right-handed individuals do not differ in ability to recognize faces, subjects arbitrarily categorized as weakly left-handed were significantly inferior at the task. Gilbert explained this result by suggesting that mixed handers have less distinct cerebral lateralization of function and thus do not possess such a clearly defined right-hemisphere dominance for face recognition. This seems a somewhat insubstantial argument, for his sample of weakly left-handed subjects scored as well as right-handers, and better than left-handers, on tests of verbal and mathematical reasoning which are largely performed (and therefore strongly lateralized) in the left hemisphere.

Yet another explanation lies in the possibility that Gilbert's group of weak left-handers contained fewer women than the other two groups. Such an imbalance of sex distribution seems likely from an inspection of the normative distributions for men and women on the test of handedness which Gilbert used. These were published by Crovitz & Zener (1962, p. 275), and reveal that slightly fewer women score in the middle ranges between absolute right- and left-handedness. In the absence of information concerning the distribution of sexes in Gilbert's groups, it remains a possibility that his data were the result of the unequal abilities of the sexes in recognizing faces (especially female ones).

Race

Strictly speaking, a discussion of the effects of a subject's race upon his ability to recognize faces belongs neither to the subject effect section nor the stimulus effect section of this review. There seems to be an interaction between race of subject and race of face. Orientals have been heard to bemoan the fact that all Westerners look alike and Westerners likewise complain about the difficulties in discriminating the faces of other races. In two recent laboratory experiments conducted in the U.S.A., white subjects were shown to be significantly better at recognizing faces of their own race compared with black faces (Cross *et al.*, 1971; Malpass & Kravitz, 1969). In both studies, however, the performance of black subjects confounded any simple hypothesis about recognition of faces from one's own race being easier: black subjects were also marginally better at white faces than black ones (as measured by d') which could be interpreted as signifying that black faces are inherently more difficult to discriminate.

Shepherd *et al.* (1974), however, examined this problem in a more natural cross-cultural setting. They then found that black Rhodesian subjects were significantly better at recognizing black compared with white faces, while Scottish subjects showed significantly superior recognition scores with white faces. This result was interpreted in terms of differential familiarity with own race faces rather than black faces being more difficult to discriminate than white faces. The fact that Cross *et al.* (1971) found no difference in their experiment as to whether a subject lived in a segregated or integrated neighbourhood, and Malpass & Kravitz (1969) could find no relationship between their white subjects' amount of self-reported experience with black people and their recognition scores on black faces, is not consistent with a simple familiarity explanation. Perhaps it is necessary for people to attach some importance to the discrimination of faces from another race before discrimination of the relevant features occurs.

In a later study, Malpass *et al.* (1973) demonstrated that training in identifying the relevant features of black faces could improve the performance of white subjects so that they became equally good at recognizing black and white faces. Similarly, Elliot *et al.* (1973) were able to improve white subjects' recognition of oriental faces by training. Related to these findings is the study by Galper (1973), who showed that white students attending courses in black studies were equally good at recognizing black and white faces, whereas other white students were better at recognizing faces of their own race. This finding led Galper to propose the concept of 'functional race membership', by which she means that strong positive attitudes towards another race can result in discrimination of faces within that race equal to that shown by actual members of the race. The concept confounds familiarity with motivational factors which, clearly, need to be differentiated.

It would also be fruitful to examine the mechanisms underlying the familiarity process itself, so that it will be possible perhaps to locate the specific facial attributes governing race differences. These might take the form of white subjects attending to such features as eye and hair colour, which are less appropriate when applied to black faces, and black subjects concentrating more on, say, face outline and skin colour, which likewise might be less discriminating cues for white than black faces. Yet another way of conceptualizing the processes involved in learning to differentiate faces from another race is to suppose that people initially attend to the salient features of a stimulus and neglect the less salient ones (the role of salience in learning is discussed by Trabasso & Bower, 1968). Thus for white subjects looking at a black face the most salient feature of the face is its colour, which does not convey much discriminating information. Experience with black faces could result in subjects learning to attend to other features which prove to be more useful discriminating cues and thereby inhibiting the tendency to devote attention to less discriminating but salient features.

OVERVIEW

One very striking aspect of the research on face recognition reviewed so far is the absence of any unifying theoretical structures. For example, it would be apposite to examine the reported age, sex and race effects from the viewpoint of perceptual learning theories, especially that involving the concept of perceptual differentiation (Gibson, 1969). Such an approach might prove useful in explaining the role of differential experience in the ability to recognize faces drawn from a particular population.

The disruption in recognition performance when faces are transformed might also be investigated within the framework of perceptual learning theories. That subjects can improve the speed with which they process inverted faces has been demonstrated

(Bradshaw & Wallace, 1971), but research should be conducted into the ways in which such an improvement might be mediated. At the same time, it would be informative for a comparison to be made of the degree of disruption in the recognition of transformed faces compared with the transformation of other complex stimuli. Rock (1974) presents data indicating that the recognition of faces, words and fragmented figures all show a fairly similar decrease in accuracy as the subject's head is tilted through 45°, 90° and 180°. According to Rock, tilted words and faces present similar cognitive difficulties, for they both require the correction of a number of separate elements before the whole figure can be recognized. When examining an inverted face, for example, the perceiver seems able only to correct a feature at a time, which leaves the remaining features uncorrected, thus producing difficulties in recognition. This analysis, it should be noted, does not treat faces as being different from any other complex pictures involving multiple components.

It would seem that research into recognition of faces could be benefited by taking account of research and theory into other pictorial material. However, there is a significant body of investigators who maintain that face recognition is a separate process to that employed for other material. If this is the case it would be possible (but not necessary) to argue that the laws governing general recognition performance need not apply to the recognition of faces. The object of the next section is to examine the bases for the assertion that faces are recognized by a special and specific set of pattern analysers.

CAN FACIAL RECOGNITION BE CONSIDERED A SPECIAL PROCESS?

The recognition of faces involves a very fast processing of information which, according to Bradshaw & Wallace (1971), is best described as a serial rather than a parallel search of the different features within the face. They base this suggestion on results obtained from experiments in which subjects rapidly searched pairs of Identi-Kit faces that varied on the number of possible different features from 2 to 7 and declared them to be either the same or different. There is some question as to the generality of this finding, for the task is rather unlike that normally involved in scanning a crowd to look for familiar faces. A more representative paradigm for establishing the everyday processing of faces would be one in which target faces (of well-known public figures perhaps) occur within an array of other faces and subjects are required to search through looking for any one of the targets. It would be no surprise to discover that the search rate for many target faces was as quick as that for one face which would support a parallel processing model. Such an experiment clearly needs to be performed, for the outcome could have profound implications on theoretical approaches to explain how faces are recognized.

Recognition of faces seems to be almost entirely a visual process. There have been attempts to explore the role of verbal coding but they have been unsuccessful in showing evidence for such mediation. Goldstein & Chance (1971), for example, discovered no relationship between the recognizability of faces and the number of verbal associations given to them; further, they noticed a marked similarity among verbal descriptions of different faces, and so doubted whether verbal mediations could do much to facilitate face recognition. A similar conclusion was drawn by Malpass *et al.* (1973) when they found that protracted verbal training on face identification did not improve visual recognition of faces, whereas a short visual training procedure led to a marked improvement in recognition performance. Verbal coding of faces is still of interest to police forces attempting to get descriptions of suspects, and computer engineers trying to design automatic procedures for identifying faces (e.g. Goldstein *et al.*, 1971), but these will not be considered further. Instead, it is intended to consider whether faces are better thought of as complex visual patterns, or as some special class of pattern which is processed by a separate, face-specific set of analysers.

The arguments in favour of the face-specific analyser hypothesis are derived from three principal sources: (*a*) the ontogeny of facial recognition; (*b*) experiments involving inversion of faces; (*c*) clinical evidence on a condition known as prosopagnosia, which is characterized by the inability of sufferers to recognize even the faces of close relatives.

Ontogeny

It is clear that infants respond to faces at a very early age (see Gibson, 1969, for an annotated review of the literature on this topic). Wolff (1963) observed that babies smiled in response to a face at about 4 weeks, but it has been suggested that even within the second week of life babies are able to differentiate their mother's face from a model of a face and an abstract model (Carpenter *et al.*, 1970). The danger when interpreting the results of this sort of research is to overlook the possibility that babies could be responding to some general difference among classes of stimuli such as brightness rather than to the pattern detail. Nevertheless, Tzavaras *et al.* (1970) have suggested that, due to the exigencies of imprinting, the rules or strategies useful to the analysis and integration of physiognomic details are innate rather than acquired: as such, faces are immediately classed differently from other visual patterns. The problem with such a hypothesis is that it is virtually impossible to prove. Even after just two weeks of life, for instance, babies will have had a relatively enormous amount of opportunity to see faces and thus learn something about their essential properties. In the same vein, there is no clear evidence for infant–mother imprinting in humans despite a great deal of interest in the topic (e.g. Bowlby, 1969).

Support for the idea of an innate proclivity for analysing faces was claimed by Sackett (1966), who found that rhesus monkeys reared in isolation from birth subsequently showed more interest in pictures of monkeys than complex scenes. His conclusion that there is an innate species-specific recognition system in monkeys, however, does not seem to be entirely warranted by his data. The monkeys would have had opportunities to see parts of their own bodies which could easily produce a preference for pictures showing bodies of other monkeys.

It would appear therefore that, at present, an analysis of the ontogeny of interest in faces cannot provide unequivocal support for the view that young infants' interest in and recognition of faces is governed by innate factors. The primate research cannot be considered unequivocal at present, but clearly provides a means for future work involving, perhaps, neurophysiological investigations.

Inversion of faces

As mentioned earlier, recognition of faces is greatly impaired by inversion (Goldstein, 1965; Hochberg & Galper, 1967). Generally speaking, compared with other classes of stimuli the recognition of faces is best in an upright orientation and worst when inverted, but this is not always the case, for Yarmey (1971) found no interaction between category of stimuli and orientation.

Yin (1969, 1970) has suggested that the extreme disruption in recognition performance for inverted faces is due to the existence of face-specific pattern analysers. Implicit in this hypothesis are two assumptions: (*a*) that some pre-processing of input occurs so that faces can be assigned to the specific analysers mentioned, and other configurations routed somewhere else for analysis; and (*b*) that the face-specific analysers are somewhat less flexible than other visual pattern analysers, in that they display a poorer capacity to cope with transformations of input (such as inversion). There is no *a priori* reason for postulating the latter assumption – in fact, Konorski (1967), who, like Yin, believes in the existence of face-specific analysers (which he terms 'gnostic units'), has made virtually the opposite assumption: namely that gnostic units for recognizing 'non-manipulable objects' (e.g. buildings and trees) are particularly orientation-sensitive. Konorski's prediction would clearly be that inverting buildings would affect recognition more than inversion of other stimuli, including faces.

How, then, can the results of Yin (1969) and Scapinello & Yarmey (1970) be accounted for? One possible approach is to examine the strategies used by people trying to recognize various types of stimuli. Yin (1969) found that many of his subjects reported employing a global or holistic strategy for faces and a more analytic or detailed strategy when looking at other material. As mentioned earlier, Rock (1974) has pointed out that when a face is inverted a number of previously familiar relationships are altered, and that any figure which involves multiple components is likely to be difficult to mentally rotate. It would seem that, until stimuli of comparable complexity and familiarity to faces can be found, the inference which Yin makes must be regarded with scepticism. We have had a great deal of practice at recognizing upright faces, and it is likely that to be asked suddenly to recognize inverted ones requires a temporary unlearning of a highly familiar strategy. Evidence that such unlearning is possible comes from the previously mentioned work of Bradshaw & Wallace (1971), who found that, with practice, subjects could significantly improve their rate of searching inverted faces.

A different type of evidence is to be found in an experiment by Goldstein (1965). He argued that children, having had less practice than adults at viewing faces, should show less of a performance decrement in a paired associate task when the pairing is letter–inverted face, compared with letter–upright face: his results confirmed the prediction but, unfortunately, a previous experiment had indicated a positive relationship between age and memory for inverted class-mates' faces (Brooks & Goldstein, 1963). It is perhaps possible to resolve these two apparently discrepant findings by noting that older children would have had much more interaction with class-mates than younger ones would have had, for they would have known them longer. Such differential familiarity could well account for the positive relationship found between age and ability to recognize inverted faces.

The inversion experiments do not appear to provide clear evidence for the existence of a face-specific recognition system. Before they can do so, much more needs to be known about the equivalence of different categories of stimuli, both from the point of view of complexity and the way in which they are typically analysed.

Prosopagnosia

Prosopagnosia is a clinical condition characterized by an inability to recognize faces. Patients suffering from prosopagnosia reveal a variety of concomitant disturbances, but all are severely impaired in recognizing faces of even themselves, their spouse, children, relatives and friends (e.g. Bay, 1953; Beyn & Knyazeva, 1962; Cole & Perez-Cruet, 1964; Macrae & Trolle, 1956; Pallis, 1955). In order to recognize people, they typically resort to using voice, clothes, gait, and sometimes distinguishing marks such as a mole: they often find difficulty in telling the faces of men from women except by features such as hair length, and a patient studied by Cole & Perez-Cruet (1964) could only distinguish black from white faces by carefully comparing facial contours and hair texture. The apparent specificity of the condition has led some to postulate the existence of face-specific analysers which are either destroyed or damaged in patients showing symptoms of prosopagnosia (Hecaen & Angelergues, 1962; Konorski, 1967; Yin, 1970). There are, however, a number of objections which can be raised to the simplicity of this idea.

First, although not essential to the hypothesis, the idea of functional specificity carries with it the assumption, which is sometimes explicit (Konorski, 1967), that the location of the function is a fixed one. It is clear, however, from the case histories mentioned that damage to a number of quite disparate cerebral areas has led to prosopagnosia. The damage has been reported, for example, in the left frontal lobe (Cole & Perez-Cruet, 1964), bilateral occipital lobes (Beyn & Knyazeva, 1962), bilateral parieto-occipital regions (Gloning *et al.*, 1966) and in the parieto-temporo-occipital junction (Hecaen & Angelergues, 1962). It is, of course, possible to have multiple sites for specific functions, and one must admit that most cases seem to arise from right-hemisphere damage (Hecaen & Angelergues, 1962), but the plurality of possible neurological areas where damage can lead to prosopagnosia could also suggest that the recognition of faces involves a number of scattered sub-processes any of which, if interfered with, can lead to a total inability to recognize faces. This is in contrast to the idea of a set of face-specific analysers situated in one region (Konorski, 1967), but is not necessarily entirely incompatible with the idea of functional specificity for analysing faces.

A second consideration when examining prosopagnosia case histories is that it is usual for patients to display symptoms of other agnosias (Gloning *et al.*, 1970). Pallis (1955), for example, noted that his patient could not identify food (e.g. an omelette and a piece of meat looked the same) and had difficulty in identifying different species of animals. Similarly, the patient described by Macrae & Trolle (1956) at first could not tell a cow from a dog and was unable to distinguish different types of cars. Colour agnosia has usually been reported along with prosopagnosia (e.g. Bay, 1953; Beyn & Knyazeva, 1962; Cole & Perez-Cruet, 1964; Gloning *et al.*, 1970; Pallis, 1955). Some patients with prosopagnosia have also shown a disturbed ability to locate themselves in space and learn new routes (Beyn & Knyazeva, 1962; Pallis, 1955). Other defects sometimes, but not usually, associated with prosopagnosia include visual field defects (Bay, 1953), constructional apraxia, dyspraxia for dressing, and metamorphosia (e.g. distortions of faces or symbols such as the red cross or swastika; Benton & Van Allen, 1968).

The sorts of difficulties with identifying faces which patients complain of include overall blurring, difficulty in interpreting the fine shades and soft forms of faces, and often an inability to infer the emotional state of a face. Some patients have reported most difficulty with the eye region (Gloning *et al.*, 1966), but for others the eyes have been the easiest feature to recognize (Hecaen & Angelergues, 1962). Whatever the actual symptoms, it is not surprising that patients with prosopagnosia are usually referred for examination because of the reported difficulty in recognizing faces. This is clearly a much more salient symptom than of any the others mentioned both to patient and clinician, and as such has merited most interest and attention (De Renzi *et al.*, 1969). It is, however, somewhat myopic to argue from the documented instances of prosopagnosia that there is a face-specific pattern recognition system when the condition is invariably associated with other symptoms.

The fact that patients can always detect a face without being able to recognize it is interesting, for it points to the existence of at least a two-stage process in which faces are first detected as such, and then undergo further analyses during which information concerning sex, age and emotional state is extracted and a match made with any stored memory trace of that particular face.

However, to say that prosopagnosia is the result of damage to some aspect of the secondary stage does not get us very far. The condition could still result from disturbance to a number of distinctly different processes. Bay (1953), for example, thought that his patient's prosopagnosia was due to his tunnel vision which interfered with the integration of the various parts of faces into wholes ('Funktionwandel'). But other patients have not suffered from tunnel vision, and evidence is not provided as to whether Bay's patient could recognize a face at a distance such that the entire configuration occurred within the tunnel range.

Faust (1947) offered an alternative but similar theory when he suggested that prosopagnosia was a failure to grasp the detailed features of a complex pattern so that its individuality was not appreciated. Neither Bay's nor Faust's theory, of course, is face-specific: Faust, in fact, reported a patient who failed not only to identify faces, but also to distinguish a chair from an armchair, indicating a more general perceptual deficit.

Pallis (1955) offered a different type of explanation for prosopagnosia when he suggested that the disability might be due to the failure of sensory input to excite an appropriate memory trace. This is a plausible explanation but entirely lacking in empirical support. It is not certain just how such support might be mustered. The rarity of prosopagnosia has meant that most studies have been conducted on a single patient. Only the investigation made by Hecaen & Angelergues (1962) reported data from a large number of prosopagnosia patients. However, their study was based on observations made on patients at different periods which did not, therefore, allow any experimental investigation.

Related experimental investigations have been made, however. These have concentrated on right *v.* left unilateral brain damage and the consensus finding is that right-hemisphere damage produces more impairment on face recognition tasks than does damage confined to the left hemisphere (Benton & Van Allen, 1968; De Renzi & Spinnler, 1966; De Renzi *et al.*, 1969; Jones, 1969; Milner, 1968; Tzavaras *et al.*, 1970; Warrington & James, 1967; Yin, 1970).

The critical question concerns the specificity of perceptual impairment arising from right-hemisphere damage. Tzavaras *et al.* (1970) believe that the deficit may be confined to recognition of faces. Their evidence is based on the finding that patients with right-hemisphere damage are particularly poor at recognizing both photographs and sketches of faces that have been disguised in a variety of ways (beard, shadow, etc.). The performances on these tasks were compared with those on rather different ones involving identification of particular stimuli from an array of similar stimuli (nonsense figures, shadow shapes and cathedrals): the right-hemisphere group was still slightly poorer than the control and left-hemisphere groups at these other tasks, but the difference was small. Unfortunately, the task difficulties were not equated. The controls, for example, made no or virtually no errors on most of the tasks except the ones involving faces. These were the ones that produced a large number of errors for the right-hemisphere group, but it is not clear whether they were just generally poor at comparatively difficult visual recognition tasks, or whether they were specifically poor at recognizing faces.

Yin (1970) showed that right posterior hemisphere damaged patients were poorer than left posterior hemisphere damaged patients and normal controls at a face memory task compared with one involving buildings as stimuli. When the stimuli were shown in an inverted orientation, however, the right posterior hemisphere group recognized more faces and about the same number of houses as the left posterior hemisphere group. In keeping with his earlier (1969) hypothesis based on normal subjects, Yin's explanation of these results is that there is a face-specific recognition system situated in the right hemisphere which is also orientation-specific. When faces are presented upside down (presumably) some other unimpaired recognition system analyses and perhaps stores the input. This explanation, however, does not account for the left posterior hemisphere group performing so badly with inverted faces. Their performance was actually worse than chance in this condition, which suggests the possibility that verbal coding is employed when stimuli are inverted and that this may be more difficult with faces than with buildings. Damage to the left hemisphere, of course, is likely to interfere particularly with any verbal coding of inverted faces.

Moreover, these findings are subject to the same considerations mentioned earlier in relation to evidence from inversion studies. Principally, if the strategy employed for viewing faces differs from that employed for viewing buildings, then what might be impaired by right-hemisphere damage is the capacity for using such a strategy. What seems to be required is the discovery of materials which are generally analysed in the same (perhaps global) way that faces are and then to repeat the experiment with inversion on brain-damaged patients.

Before leaving this topic, it is worth pointing out that De Renzi & Spinnler (1966) showed that patients with right-hemisphere damage were poorer than those with left-hemisphere damage at recognizing not only faces but also chairs and abstract figures. Performance on the faces and abstract figures tests correlated significantly which led De Renzi & Spinnler to speculate that individuals with right hemisphere damage were deficient in the high-level integration of visual data. In support of this view, Jones (1969) has found that right-hemisphere damage can impair visual search efficiency, which in turn could affect integration of features. Such a deficiency is clearly not face-specific and, as mentioned earlier, could be monitored by using other complex material, which perhaps tends to be viewed in a global rather than an analytic manner.

CONCLUSIONS

There does not seem to be any unambiguous evidence that faces are handled by a special and specific recognition system. The existence of such a system could have certain adaptive benefits, but could also present the visual system with problems of integrating different aspects of a scene – especially fitting the analysis of the face to the analysis of the body.

Faces obviously constitute a rather special class of visual input because of their complexity, the familiarity with which we experience them, the motivation which we have to remember them, and the ways in which they can convey non-verbal information. But as yet there is no reason to suppose that the recognition and storage of faces basically differs from that involved when other visual information is presented. Indeed, the ability of faces to interfere retroactively with the memory of geometrical patterns has already been established (Cohen & Granström, 1970), suggesting some communality of processing for both types of material.

The upshot of this conclusion is that facial recognition is likely to be subject to similar independent variables as those affecting the recognition of other complex visual stimuli.

REFERENCES

BAY, E. (1953). Disturbances of visual perception and their examination. *Brain* **76**, 515–550.

BENTON, A. L. & VAN ALLEN, M. W. (1968). Impairment of facial recognition in patients with cerebral disease. *Cortex* **4**, 344–358.

BEYN, E. S. & KNYAZEVA, G. R. (1962). The problem of prosopagnosia. *J. Neurol. Neurosurg. Psychiat.* **25**, 154–158.

BOWLBY, J. (1969). *Attachment*, vol. 1. Harmondsworth: Penguin Books.

BRADSHAW, J. L. & WALLACE, G. (1971). Models for the processing and identification of faces. *Percept. Psychophys.* **9**, 443–448.

BROOKS, R. M. & GOLDSTEIN, A. G. (1963). Recognition by children of inverted photographs of faces. *Child Dev.* **34**, 1033–1040.

CARPENTER, G. C., TECCE, J. J., STECHLER, G. & FRIEDMAN, S. (1970). Differential visual behaviour to human and humanoid faces in early infancy. *Merrill-Palmer Q.* **16**, 91–108.

COHEN, R. L. & GRANSTRÖM, K. (1970). Reproduction and recognition in short-term visual memory. *Q. J. exp. Psychol.* **22**, 450–457.

COLE, M. & PEREZ-CRUET, J. (1964). Prosopagnosia. *Neuropsychol.* **2**, 237–245.

CROSS, J. F., CROSS, J. & DALY, J. (1971). Sex, race, age and beauty as factors in recognition of faces. *Percept. Psychophys.* **10**, 393–396.

CROVITZ, H. & ZENER, C. (1962). A group test for assessing eye and hand dominance. *Am. J. Psychol.* **75**, 271–275.

DEREGOWSKI, J. B., ELLIS, H. D. & SHEPHERD, J. (1973). A cross-cultural study of recognition of pictures of faces and cups. *Int. J. Psychol.* **8**, 269–273.

DE RENZI, E., SCOTTI, G. & SPINNLER, H. (1969). Perceptual and associative disorders of visual recognition. *Neurology* **19**, 634–642.

DE RENZI, E. & SPINNLER, H. (1966). Facial recognition in brain-damaged patients. *Neurology* **16**, 145–152.

EKMAN, P., FRIESEN, W. V. & ELLSWORTH, P. (1972). *Emotion in the Human Face*. Oxford: Pergamon Press.

ELLIOTT, E. S., WILLS, E. J. & GOLDSTEIN, A. G. (1973). The effects of discrimination training on the recognition of white and oriental faces. *Bull. psychon. Soc.* **2**, 71–73.

Ellis, H. D., Shepherd, J. & Bruce, A. (1973). The effects of age and sex upon adolescents' recognition of faces. *J. genet. Psychol.* **123**, 173–174.

Ellis, H. D., Shepherd, J. & Davies, G. M. (1975). An investigation of the use of the Photo-fit technique for recalling faces. *Br. J. Psychol.* **66**, 29–37.

Fagan, J. F. (1972). Infants' recognition memory for faces. *J. exp. child Psychol.* **14**, 453–476.

Fagan, J. F. (1973). Infants' delayed recognition memory and forgetting. *J. exp. child Psychol.* **16**, 424–450.

Faust, C. (1947). Partielle Seelenblindheit nach Occipitalverletzung mit besonderer Beeinträchtigung des Physiognomieerkennens. *Nervenarzt* **18**, 294–297.

Galper, R. E. (1970). Recognition of faces in photographic negative. *Psychon. Sci.* **19**, 207–208.

Galper, R. E. (1973). 'Functional race membership' and recognition of faces. *Percept. mot. Skills* **37**, 455–462.

Galper, R. E. & Hochberg, H. (1971). Recognition memory for photographs of faces. *Am. J. Psychol.* **84**, 351–354.

Gibson, E. J. (1969). *Principles of Perceptual Learning and Development.* New York: Appleton-Century-Crofts.

Gilbert, C. (1973). Strength of left-handedness and facial recognition ability. *Cortex* **9**, 145–151.

Gilbert, C. & Bakan, P. (1973). Visual asymmetry in perception of faces. *Neuropsychol.* **11**, 355–362.

Gloning, I., Gloning, K., Hoff, H. & Tschabitscher, H. (1966). Zur Prosopagnosie. *Neuropsychol.* **4**, 113–131.

Gloning, I., Gloning, K., Jellinger, K. & Quatember, R. (1970). A case of 'prosopagnosia' with necropsy findings. *Neuropsychol.* **8**, 199–204.

Goldstein, A. G. (1965). Learning of inverted and normally oriented faces in children and adults. *Psychon. Sci.* **3**, 447–448.

Goldstein, A. G. & Chance, J. (1964). Recognition of children's faces. *Child Dev.* **35**, 129–136.

Goldstein, A. G. & Chance, J. (1971). Visual recognition memory for complex configurations. *Percept. Psychophys.* **9**, 237–241.

Goldstein, A. G. & Chance, J. (1974). Some factors in picture recognition memory. *J. gen. Psychol.* **90**, 69–85.

Goldstein, A. G. & Mackenberg, E. G. (1966). Recognition of human faces from isolated facial features: a developmental study. *Psychon. Sci.* **6**, 149–150.

Goldstein, A. J., Harmon, L. D. & Lesk, A. B. (1971). Identification of human faces. *Proc. IEEE* **59**, 748–760.

Hecaen, H. & Angelergues, R. (1962). Agnosia for faces (prosopagnosia). *Archs Neurol.* **7**, 92–100.

Hochberg, J. & Galper, R. E. (1967). Recognition of faces. I. An exploratory study. *Psychon. Sci.* **9**, 619–620.

Hochberg, J. & Galper, R. E. (1974). Attribution of intention as a function of physiognomy. *Memory & Cogn.* **2**, 39–42.

Howells, T. H. (1938). A study of the ability to recognize faces. *J. abnorm. soc. Psychol.* **33**, 124–127.

Hutt, C. (1972). *Males and Females.* Harmondsworth: Penguin Books.

Jones, A. C. (1969). Influence of mode of stimulus presentation on performance in facial recognition tasks. *Cortex* **5**, 290–301.

Kinsbourne, M. (1973). The control of attention by interaction between the cerebral hemispheres. In S. Kornblum (ed.), *Attention and Performance*, vol. 4. New York: Academic Press.

Kleinsmith, L. J. & Kaplan, S. (1963). Paired associate learning as a function of arousal and interpolated interval. *J. exp. Psychol.* **65**, 190–193.

Konorski, J. (1967). *Integrative Activity of the Brain.* Chicago: University of Chicago Press.

Laughery, K. R., Alexander, J. F. & Lane, A. B. (1971). Recognition of human faces: effects of target exposure time, target position, pose position and type of photograph. *J. appl. Psychol.* **55**, 477–483.

Macrae, D. & Trolle, E. (1956). The defect of function in visual agnosia. *Brain* **79**, 94–110.

McCurdy, H. G. (1949). Experimental notes on the asymmetry of the human face. *J. abnorm. soc. Psychol.* **44**, 553–555.

McKelvie, S. J. (1972). Strategies of encoding in memory for faces. (Ph.D. thesis, McGill University.)

Malpass, R. S. & Kravitz, J. (1969). Recognition for faces of own and other 'race'. *J. Person. soc. Psychol.* **13**, 330–335.

Malpass, R. S., Lavigueur, H. & Weldon, D. (1973). Verbal and visual training in face recognition. *Percept. Psychophys.* **14**, 285–292.

Messick, S. & Damarin, F. (1964). Cognitive styles and memory for faces. *J. abnorm. soc. Psychol.* **69**, 313–318.

Milner, B. (1968). Visual recognition and recall after right temporal-lobe excision in man. *Neuropsychol.* **6**, 191–209.

Nickerson, R. S. (1965). Short-term memory for complex, meaningful, visual configurations: a demonstration of capacity. *Can. J. Psychol.* **19**, 155–160.

Noton, D. & Stark, L. (1971). Eye movements and visual perception. *Scient. Am.* **224**, 35–43.

Pallis, C. A. (1955). Impaired identification of faces and places with agnosia for colours. *J. Neurol. Neurosurg. Psychiat.* **18**, 218–224.

Peters, A. (1917). Gefühl und Wiederkennen. *Fortschr. Psychol.* **4**, 120–133.

Phillips, R. J. (1972). Why are faces hard to recognize in photographic negative? *Percept. Psychophys.* **12**, 425–426.

Rock, I. (1974). The perception of distorted figures. *Scient. Am.* **230**, 78–85.

Sackett, G. P. (1966). Monkeys reared in isolation with pictures as visual input: evidence for innate releasing mechanism. *Science, N.Y.* **154**, 1470–1473.

Scapinello, K. F. & Yarmey, A. D. (1970). The role of familiarity and orientation in immediate and delayed recognition of pictorial stimuli. *Psychon. Sci.* **21**, 329–330.

Schill, T. R. (1966). Effects of approval motivation and varying conditions of verbal reinforcement on incidental memory for faces. *Psychol. Rep.* **19**, 55–60.

Shepherd, J. W., Deregowski, J. B. & Ellis, H. D. (1974). A cross-cultural study of recognition memory for faces. *Int. J. Psychol.* **9**, 205–212.

Shepherd, J. W. & Ellis, H. D. (1973). The effect of attractiveness on recognition memory for faces. *Am. J. Psychol.* **86**, 627–634.

Smith, E. E. & Nielsen, G. D. (1970). Representations and retrieval processes in STM recognition and recall of faces. *J. exp. Psychol.* **85**, 397–405.

Trabasso, T. & Bower, G. H. (1968). *Attention in Learning.* New York: Wiley.

Tzavaras, A., Hecaen, H. & Lebras, H. (1970). Le problème de la spécificité du déficit de la reconnaissance du visage humain lors des lésions hémisphérique-unilatérales. *Neuropsychol.* **8**, 403–416.

Warrington, E. K. & James, M. (1967). An experimental investigation of facial recognition in patients with unilateral cerebral lesions. *Cortex* **3**, 317–326.

Witkin, H. A., Dyk, R. B., Faterson, H. R., Goodenough, D. R. & Karp, S. A. (1962). *Psychological Differentiation: Studies of Development.* New York: Wiley.

Witryol, S. L. & Kaess, W. A. (1957). Sex differences in social memory tasks. *J. abnorm. soc. Psychol.* **54**, 343–346.

Wolff, P. (1963). Observations on the early development of smiling. In B. M. Foss (ed.), *Determinants of Infant Behaviour II.* London: Methuen.

Yarmey, A. D. (1971). Recognition memory for familiar 'public' faces: effects of orientation and delay. *Psychon. Sci.* **24**, 286–288.

Yin, R. K. (1969). Looking at upside-down faces. *J. exp. Psychol.* **81**, 141–145.

Yin, R. K. (1970). Face recognition by brain-injured patients: a dissociable ability? *Neuropsychol.* **8**, 395–402.

Zusne, L. (1970). *Visual Perception of Form.* New York: Academic Press.

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THE SEMANTICS OF LOCATIVE INFORMATION IN PICTURES AND MENTAL IMAGES

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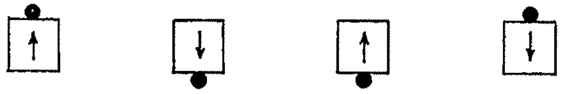
Three experiments examined how people compare sentences about spatial location to pictures and images. Previous investigations have found that people are faster at judging relative location when the description contains the word *above* or *right* than when it contains the word *below* or *left*. Expt. I showed that this asymmetry persisted when the words were replaced by arrows, indicating that the effect is not specific to particular lexical items. Expt. II showed the asymmetry persisted even when the response latency did not include the time to encode the description, indicating that the asymmetry does not lie in the description-encoding stage. Finally, Expt. III investigated how people compare sentences to information from a previously memorized picture. In this situation, the usual asymmetry was not present. The three studies suggest that the asymmetry arises from the way descriptions influence the encoding of perceptual events. The results also showed that the information encoded in a mental representation of a picture is ordered such that certain features can be accessed more quickly than others. However, the same features are equally quickly accessed in a picture that is physically present.

Judging the relative location of objects in a visual display is easier when the display is described in terms of the word *above* than when it is described in terms of *below* (Seymour, 1969; Chase & Clark, 1971). For example, people are faster to confirm that the word *above* correctly describes an object in an upper location than confirming that *below* correctly describes a lower location. Similarly, Olson & Laxar (1973) have found that verification of descriptions with the word *right* is faster than verification of *left*. Performance in these word (or sentence)–picture verification tasks has been characterised in terms of four independent stages whose durations are additive (Chase & Clark, 1971): (1) reading and encoding the verbal description, (2) scanning and encoding the picture, (3) comparing the representation of the description with the representation of the picture, and (4) executing the result of the comparison stage (true or false) as an overt response. The processing asymmetries between *above* and *below* (and *right* and *left*) have been ascribed to various stages, such as the reading and encoding of the description (Chase & Clark, 1971; Olson & Laxar, 1973), the scanning and encoding of the picture (Seymour, 1969) or the effect of the first three stages on response availability (Seymour, 1974). The present paper will demonstrate that the asymmetry between *above* and *below* (and *right* and *left*) is not specifically linguistic, but results from a more general asymmetrical conception of spatial dimensions. The experimental results are used to develop a model for the processing of relative spatial location which accounts for the asymmetry.

A secondary purpose of this paper is to compare the way information is retrieved from a picture to the way it is retrieved from a mental representation of that picture. The study of locatives (such as *above* and *below*) inherently involves the spatial distribution of information, one of the defining properties of pictures and images. The comparison of information retrieval from pictures and mental images will arise in Expts. II and III. The two experiments are identical except that in Expt. II sentences are verified as true or false with respect to a picture, while in

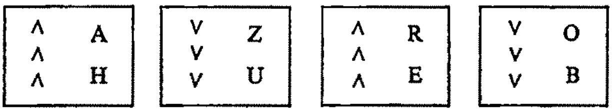
Table 1. *Schematic diagram of displays*

(*a*) Displays used by Chase & Clark (1971)



Arrow:	'above'	'below'	'above'	'below'
Response:	yes	yes	no	no

(*b*) Displays used in Expt. I



Arrow:	'above'	'below'	'above'	'below'
Response:	yes	yes	no	no

Expt. III the same sentences are verified with respect to a mental image of that same picture. A cognitive theory which copes with imagery must be able to specify the properties of an image – the kind of information that is stored, the format of the information, and the kinds of mental operations that can be applied to the information. Perhaps most importantly, the theory must specify how these properties are different for imaginal and non-imaginal representations. The comparison of performance between Expts. II and III provides some of the preliminary data about the relative accessibility of information in pictures and mental images.

There have been several kinds of theories to explain the asymmetries like those for *above* and *below* in word–picture comparison tasks. One of these theories has attributed the difference to processes in the word-encoding stage. Chase & Clark (1971) and Olson & Laxar (1973) argued that the words *below* and *left* take longer to encode than *above* and *right*, respectively. The proposal was that *below* and *left* refer to the negative pole of their underlying dimension, and this negativity takes extra time to encode.

The strongest evidence in favour of the word-encoding explanation of the asymmetry comes from a verification task which was identical to the *above-below* study, except that the words *above* and *below* were replaced with the corresponding non-linguistic symbols (↑) and (↓) (Chase & Clark, 1971). An example of the displays is shown in Table 1. This procedure eliminated the word-encoding process, and the *above–below* (or↑↓) latency difference disappeared. Similarly, Olson & Laxar (1973) eliminated the *right–left* difference when the words were replaced by the symbols (→) and (←). The conclusion drawn in these two papers was that the advantage of *above* and *right* had been due to the word encoding process.

It is possible that subjects in these 'arrow' experiments could have responded on the basis of perceptual properties of each display. For example, the decision rule could have been 'Respond *true* if the arrowhead is close to the dot; otherwise respond *false*'. Subjects may not have compared the 'meaning 'of the arrow to the representation of the location of the dot. Rather, they might have responded on the basis of global perceptual features. If subjects were basing their response on such perceptual properties, then the entire process is very dissimilar to the usual word–picture comparison task. The use of these particular displays may have eliminated both the word-encoding stage and the subsequent process that compares the two representations. While these 'arrow' experiments led to the conclusion that the asymmetry lay in the word-encoding stage, the alternative interpretation of the experiments leaves that conclusion in doubt.

EXPERIMENT I

The purpose of Expt. I was to investigate whether there was any latency advantage in favour of a non-linguistic symbol that denotes an upper location. Like Chase & Clark (1971), we used arrows to denote *above* and *below*. However, the arrow in the Chase & Clark experiment was strongly asymmetrical, with the direction of the asymmetry defining its meaning. This structural property of the arrow itself, rather than its meaning, may have been a perceptual cue that people used to make their decisions. To diminish this type of cue, we used arrowheads as shown in the lower part of Table 1, paired with a consonant and a vowel, one letter above the other. Our intention was to force subjects to encode the direction of the arrow, encode the location of the vowel, and then compare the two locations. The subjects' task was to decide whether the direction of the arrow (up or down) was the same as the location of the vowel.

The hypothesis was that people would have to explicitly encode the direction in which the arrowheads were pointing, and compare this direction to the encoded location of the vowel. If the usual advantage of the word *above* is in some process other than word-encoding, i.e. either in picture encoding or in the comparison process, then the upward-pointing arrows should enjoy a similar advantage. If the results show this advantage, then it would follow that the marking advantage of *above* over *below* may not lie in the word-encoding stage.

Method

The experiment was a timed verification task. The subject first examined an arrow that pointed up or down, as shown in Table 1. Then he examined an accompanying display of a consonant and a vowel to determine whether the vowel was above or below the consonant. If the direction of the arrow corresponded to the location of the vowel, the subject responded 'yes'; if it did not, he responded 'no'.

Stimuli

The letters in the display were 9° of visual angle to the right of arrow. The letter arrays were formed by randomly pairing one of the vowels AEIOU with one of the consonants BHLRZ. There were five exemplars of each condition, composed of five different vowel-consonant pairs. There were four experimental conditions: arrow pointing up or down, and vowel above or below the consonant. Thus there were 20 different stimuli.

Procedure

The subject initiated a trial by pressing a microswitch. Half a second later, the display appeared in the tachistoscope. The subject was instructed to look first at the arrow and then at the letters. The subject responded either 'yes' or 'no' by pressing one of two response buttons. The assignment of dominant hand to response button was balanced across subjects.

Subjects were instructed as to which stimuli were 'matching' and which 'mismatching', but

Table 2. *Response latencies in msec. (percentage error) for Expt. I*

	Stimulus description	
Response	Arrow pointing upward	Arrow pointing downward
Yes	1111 (2·1 %)	1187 (2·5 %)
No	1230 (2·7 %)	1265 (1·0 %)
Mean	1170	1226

the words *above* and *below* were never used. Prior to the beginning of the experiment, the subject was given eight practice trials, as well as four lead-in trials before each block of trials. Feedback as to the correctness of responses was given only during practice trials. The subject then went through eight blocks of 20 test trials. The order of trials within blocks was random and different for each subject. The subjects were 12 students who were paid for their participation.

Results

The analysis showed that latencies were shorter when the arrow pointed up than when it pointed down, as shown in Table 2. The data were collapsed over the five or fewer correct responses for each condition. An analysis of variance examined the effects of the upward or downward pointing arrows, the matching or mismatching displays, and the three blocks of practice. The analysis showed that the advantage of upward-pointing arrows was 56 msec. (S.E. = 17 msec.) ($F = 10{\cdot}32$; d.f. = 1,11; $P < 0{\cdot}01$). The results also showed that matching (yes) trials were faster by 98 msec. ($F = 15{\cdot}21$; d.f. = 1,11; $P < 0{\cdot}01$). Also, subjects responded faster with increased practice ($F = 5{\cdot}68$; d.f. = 7,77, $P < 0{\cdot}01$), although practice did not interact with the other effects. None of the interactions were significant. The overall error rate was a low 2·1 per cent.

The results of Expt. I show that people are generally faster when they process the concept of upwardness than when they process the concept of downwardness. The words *above* and *below* were not used in the experiment. Thus, the advantage of upwardness is not specific to lexical items like *above*. Rather it may take less time to encode a symbol denoting upwardness than to encode one that denotes downwardness.

This interpretation is only slightly constrained by the fact that the task involved the position of a vowel relative to a consonant. Vowel localization is a new task that may have characteristics of its own. For example, Posner & Mitchell (1967) found that decisions that two letters belong to the same category (either vowels or consonants) were faster when the letters were vowels. If a vowel effect does happen to interact with location, then it does so in a way that preserves the upwardness advantage. The presence of this advantage, in the absence of verbal symbols such as *above* or *below*, fulfils the major purpose of Expt. I.

The current result suggests that the advantage of upwardness does not lie in the encoding of words *per se*. However, the results do not indicate at what stage the advantage does occur. It could be occurring in the initial encoding of the meaning of the arrows, the encoding of the subsequent consonant–vowel display, the comparison process, or a response process. Expt. II examined whether the advantage persists when the duration of the first stage is eliminated from the response latency.

EXPERIMENT II

The purpose of Expt. II was to show that the advantage of the word *above* was *not* in the initial encoding stage. The experiment uses a methodology that removes the word-encoding process from the response latency, without altering the nature of the sentence–picture comparison task. Very simply, the person heard and encoded a sentence, indicated that he had comprehended it, and 500 msec. later a picture appeared. The person decided whether the sentence was true or false of the picture. Response latency was timed *only from the onset of the picture*. Thus the latency should not include the time to hear and encode the sentence. If in this paradigm the latencies are shorter for *above* than for *below* and shorter for *right* than for *left*, it would indicate that the asymmetry between the terms is due to a stage of processing later than word encoding.

Expt. II also examined the way information is retrieved from a picture. People verified sentences about a simple picture containing four consonants, each at the vertex of a rectangle. Even this simple display allows us to determine whether information from certain parts of the display is more quickly accessed than from other parts. For example, suppose the picture is $\begin{smallmatrix} W & X \\ Y & Z \end{smallmatrix}$. The test sentence can ask if W is to the left of X, or if Y is to the left of Z. Both these statements require the response 'yes', and both contain the same preposition, *left*. They differ only in whether they refer to the top or bottom part of the picture. If the statements concerning whether W is left or right of X are verified faster than the questions concerning Y and Z, the inference can be made that the top of the display is more accessible than the bottom part. Similarly, questions can interrogate whether W is above or below Y and whether X is above or below Z. If the *above-below* statements involving W and Y are verified faster, it would suggest that the left side of the picture is more accessible than the right. Thus this procedure will show whether one part of the picture is more accessible than another without confounding the meaning of the sentence with its referent.

Method

The experiment was a verification task in which the subject listened to a sentence like *W is below Y*. Then he was timed while he looked at a picture and decided whether the sentence was true or false of the picture.

Stimuli

The pictures consisted of four letters arranged to form the vertices of a rectangle or square. There were four possible figures, with dimensions (in cm) 8 × 8, 8 × 3, 3 × 8 and 3 × 3. They were viewed in a tachistoscope at a distance of 58·1 cm so that 8 and 3 cm subtended visual angles of approximately 8° and 3°, respectively. The letters in the figures were four consonants randomly chosen from 16 possible consonants. A different quadruplet of consonants was assigned to each shape. The position of the four consonants varied from trial to trial, with the constraint that in all variations, a pair of letters was always on the same axis. For example, in all pictures W was either above or below Y, and W was either to the left or right of X. This constraint allows four possible arrangements of consonants for each shape.

The sentences were of three types: *X is above/below Y*, *X is to the right/left of Y*, and *X is diagonally opposite Y*. The diagonally opposite sentences were included to discourage people from treating the consonants as two ordered pairs. The importance of this will become apparent in the next experiment. Moreover, a sentence with *above* or *below* could refer to the letters on the left of the figure or on the right. Analogously, a sentence with *left* or *right* could refer to letters on

Table 3. *Response latencies in msec. (percentage error) for Expt. II*

	Stimulus sentence			
Response	Above	Below	Right	Left
	(Letters on the right side)		(Letters on the top)	
True	1106 (1·0)	1127 (2·1)	1233 (5·2)	1210 (7·3)
False	1123 (2·1)	1122 (1·0)	1168 (2·1)	1439 (9·4)
	(Letters on the left side)		(Letters on the bottom)	
True	1052 (2·1)	1187 (5·2)	1181 (4·2)	1340 (7·3)
False	1122 (0·0)	1192 (0·0)	1278 (7·3)	1250 (5·2)
Mean	1101	1157	1214	1309

the top or on the bottom of the figure. Finally a sentence with *diagonally opposite* could refer to either of the two diagonals, and could name the letters from upper to lower, or vice versa. Thus there were 12 true sentences associated with each shape. In addition, 12 false sentences were constructed by reversing the position of the two letters in the true sentence. For example, if it was true that X *is above* Y, then the false counterpart was Y *is above* X. Sentences with *diagonally opposite* were falsified by citing two of the letters that were not diagonally related.

Procedure

On each trial, the experimenter read a sentence to the subject, like X *is above* Y. The subject pushed a button as soon as he had understood the sentence. Half a second later, the picture was presented. The subject was timed from the onset of the picture until he responded true or false. Therefore the response latency did not include the time to hear and represent the sentence. The subject indicated his response by pushing one of two response buttons. The assignment of dominant hand to response was balanced across subjects.

Prior to the test trials, the subject was given 24 practice trials on a test figure, 6 × 3 cm, composed of four vowels. Then he was given two blocks of 24 test trials where all the sentences were followed by pictures of a particular shape, for example, only 8 × 8 cm squares, all containing the same four consonants, but not always in the same arrangement. Then the next two blocks of trials dealt with pictures of a different shape, and so on, until the subject had gone through each of the four shapes. Thus there was a total of 192 trials. The order of the 24 sentences was randomized within and between blocks. Also, each subject went through the four shapes in a different order. The subjects were 12 right-handed college students who were paid for their participation.

Results

The data for the correct responses were averaged over the two blocks of trials for each shape. The latencies for questions about vertical relations (*above* or *below*) were analysed separately from statements about horizontal relations (*left* and *right*). The latencies for questions about diagonal relations were not analysed because they were irrelevant to the theoretical issues of interest here.

Vertical relations. The latencies for the correct responses were submitted to an analysis of variance, whose factors were the four shapes, the two truth values, the marked (*below*) and unmarked (*above*) adjective, and the position of the interrogated items (i.e. the left or right side of the rectangle). Only one term in the analysis of variance was significant, and that was the term of major interest. Sentences with *above* were verified an average of 56 msec. (S.E. = 23 msec.) faster than sentences with *below* ($F = 6{\cdot}12$; d.f. = 1,11; $P < 0{\cdot}05$). These results are shown in Table 3.

Horizontal relations. Sentences with *right* were verified an average of 95 msec. (S.E. = 41 msec.) faster than sentences with *left* ($F = 5{\cdot}41$; d.f. = 1,11; $P < 0{\cdot}05$).

Apart from this term, three interaction terms reached significance. The *right–left* difference was larger for the two wider shapes ($F = 4{\cdot}40$; d.f. = 3,33; $P < 0{\cdot}05$). There was a three-way interaction between true–false, *right–left*, and top–bottom positions ($F = 7{\cdot}24$; d.f. = 1,11; $P < 0{\cdot}05$). There was also a four-way interaction between the three factors just mentioned and the four shapes ($F = 4{\cdot}93$; d.f. = 3,33; $P < 0{\cdot}05$).

One important point to notice is that the position of the interrogated item had no main effect on latencies. It did not matter whether the items were on the left or on the right (for *above* or *below* sentences), and whether they were on the top or bottom (for *left* or *right* sentences). So it appears that the various locations of the picture are equally quickly accessible.

Discussion

The size of the marking effect in this experiment, i.e. the advantage of *above* over *below*, and *right* over *left*, is similar to those found in experiments that included the time for word-encoding. This experiment attempted to eliminate the word-encoding stage by instructing subjects to initiate the picture presentation after they 'understood' the verbally presented sentence. And beyond that, the subjects were given an additional $\frac{1}{2}$ sec. before the picture appeared. Thus subjects were probably given sufficient time to encode the sentence. Even so, this experiment resulted in a 56 msec. marking effect for *above–below* and 95 msec. for *right–left*. We can compare this effect to that found in other experiments. Clark & Chase (1972) had people verify sentences like *The star is(n't) above/below the plus* and found a marking effect of 93 msec. In experiments where words (*above/below*) were compared with the location of a circle in a display like $\overset{\circ}{\square}$, the marking effect was 75 msec. (Chase & Clark, 1971) and 53 msec. (Seymour, 1969). Similarly, the 95 msec. preference for the term *right* in the current experiment is very similar to the 94 msec. preference found by Olson & Laxar (1973) in their word–picture comparison task. Thus, even though the latencies in this experiment exclude sentence reading and encoding time, the marking effect is still present. Moreover, it is of the same magnitude as the marking effects found in experiments, like those cited above, that do include reading and representation time. Thus the advantage of *above* and *right* does not seem to lie in the word-encoding stage.

EXPERIMENT III

Comprehension and verification of sentences entail the comparison of information from the sentence to information from another source. The latencies necessarily include the time to scan and encode the picture. But the comprehension and verification process can be assessed in the absence of picture scanning and encoding in cases where the information from the sentence is compared to information already stored in long-term memory (Just, 1974). In Expt. III people became very familiar with a simple picture (very similar to the ones used in Expt. II) and then timed as they decided whether statements about the memorized picture were true or false. Thus the task contains all the stages of the verification process, except that the picture scanning and encoding stage is replaced by the stage that retrieves information about the picture from long-term memory. This experiment should indicate the similarities and differences between retrieving information from a picture and retrieving information from a memorial representation of the picture.

Method

The experiment was a verification task in which subjects were timed as they read a sentence like *X is above Y* and decided whether it was true or false of a picture they had previously studied. The design of the experiment was similar to that of Expt. II. The main difference was that the arrangement of the quadruplet of consonants in the picture of a given shape was kept constant so that subjects could memorize the picture and respond to the sentence on the basis of their memory.

Stimuli

The pictures were of the same size and shape as in Expt. II, and presented at the same visual angle. However, the four consonants assigned to a given shape were kept in a single arrangement. The sentences were exactly the same as in Expt. II, but presented visually.

Procedure

The subject was given a test figure and asked to form a mental image of it since he would have to verify statements about the figure. The subject studied each figure until he was certain he had memorized it. The purpose of including statements about diagonal relations was to encourage imaginal coding. The order of the 24 sentences was randomized for each block and subject. After each block of 24 trials the subject was asked to recall the test figure by drawing it exactly as he had seen it. There were two blocks of trials for each shape. The subject memorized and verified sentences about one shape at a time. The order in which the four shapes were examined was different for each subject.

The subject initiated a trial by closing a microswitch. Half a second later a sentence was presented and the subject was timed until he responded whether it was true or false of the memorized picture.

The subjects were 12 undergraduates who participated as part of an introductory psychology requirement. Eleven of the subjects were right-handed.

Results

Vertical relations. The results were treated exactly as in Expt. II. Of the four factors and their interaction terms, there were only two significant effects. People answered faster (by about 404 msec.) when they were asked about letters on the left side of the figure than when they were asked about the right side ($F = 19{\cdot}28$; d.f. = 1,11; $P < 0{\cdot}01$), as shown in Table 4. This left–right difference cannot be due to the comprehension of the sentences, since the sentences were the same and, moreover, referred to vertical rather than horizontal relations.

The second significant effect was an interaction between *above–below* and true–false ($F = 37{\cdot}80$; d.f. = 1,11; $P < 0{\cdot}01$). *Above* was verified faster when it was true, while *below* was verified faster when it was false.

Horizontal relations. The data were treated analogously to the vertical relations, and the analysis yielded precisely analogous results. Statements about letters on the top were verified about 355 msec. faster than statements about the bottom ($F = 12{\cdot}37$; d.f. = 1,11; $P < 0{\cdot}01$). The *left–right* × true–false interaction was also present ($F = 13{\cdot}75$; d.f. = 1,11; $P < 0{\cdot}01$). Sentences with the preposition *left* were verified faster when they were true, while sentences with the preposition *right* were faster when false. Even though there were many conditions in this experiment, the results can be characterized in terms of two factors, the interaction term and the position of the interrogated items.

The difference between *above* and *below* was not significant ($F = 3{\cdot}39$; d.f. = 1,11).

Table 4. *Response latencies in msec. (percentage error) for Expt. III*

	Stimulus sentence			
Response	Above	Below	Right	Left
	(Letters on the right side)		(Letters on the top)	
True	3095 (3·1)	3916 (6·3)	3964 (4·2)	3346 (3·1)
False	3649 (0·0)	3357 (1·1)	3660 (3·1)	4223 (4·2)
	(Letters on the left side)		(Letters on the bottom)	
True	2662 (3·1)	3490 (3·1)	4460 (9·4)	3592 (5·2)
False	3416 (0·0)	2835 (1·1)	4068 (2·1)	4492 (8·3)
Mean	3206	3400	4038	3913

Only 8 of the 12 subjects were faster on *above*, but one of them by over 1000 msec. The difference between *left* and *right* (*left* being slightly faster) was also not significant ($F = 1{\cdot}16$; d.f. = 1,11). These results can be compared to those of Expt. II. In Expt. II the advantage of *above* over *below* was 56 msec., with an S.E. of 23 msec., while the advantage of *right* over *left* was 95 msec. with an S.E. of 41 msec. In Expt. III, which had the same sentences, the same number of subjects and the same number of observations, the advantage of *above* was 192 msec., but was highly variable, with an S.E. of 105 msec. Furthermore, there was no advantage of *right*; in fact, *left* became faster by 125 msec., but this difference was highly variable, with an S.E. of 116 msec. So the sentence–picture experiment, which does not include sentence-encoding time, shows the usual small but reliable advantage of *above* and *right*. The experiment that referred to pictures in long-term memory, where latencies did include sentence-encoding time, showed large and unstable effects.

The response latencies were more variable and about three times longer in Expt. III than in Expt. II. It is possible that some of this extra duration and variability in Expt. III is due to a visualization conflict of the kind reported by Brooks (1967). Reading a sentence may interfere with maintaining or scanning the representation of the array of letters. After the subject read the sentence, it may have been necessary for him to retrieve the information about the array from long-term memory, and this retrieval process might produce the top–down and left–right positional effects that were obtained. Interference of this sort could potentially introduce the kind of variability and longer latencies that were observed, and these effects could have masked a small marking effect.

The overall error rate was 3·6 per cent similar to the 3·8 per cent in Expt. II. The data from the one left-handed subject were much like everyone else's.

Discussion

The results demonstrated that the small but reliable advantage of *above* found in previous studies becomes highly variable and, for *left–right*, reverses direction when people retrieve information about pictures from long-term memory. The absence of a reliable advantage suggests that perhaps the effect is due primarily to picture encoding, the stage that was eliminated with the present procedure.

Both of the major results of Expt. III can be explained by assuming that subjects represented the information from the picture in a particular order: from top to bottom and from left to right. One major result was that sentences with *above* were verified faster when true, but those with *below* were faster when false. What these two faster conditions have in common is that the first consonant in the probe sentence is the upper one in the picture. Since this condition was faster it may be inferred that the consonants in the picture were represented from top to bottom. For example, suppose the left-hand column in the picture $\begin{smallmatrix}W & X\\ Y & Z\end{smallmatrix}$ was encoded as the proposition *W is above Y*. The coding matches the order of the consonants in the true sentence *W is above Y*, as well as the false sentence *W is below Y*. And latencies to both were short, presumably because the order of the consonants in the sentence matched the represented order from the picture. However, subjects took much longer when the order of the consonants in the sentence did not match the top-to-bottom order in the picture. They were equally slow for the true sentence *Y is below W* and for the false sentence *Y is above W*. One possibility is that when the order of the consonants in the sentence did not match the top-to-bottom order, subjects recoded their representation of the picture or the sentence and then recompared them. A similar explanation accounts for the true–false × *left–right* interaction if it is assumed that a horizontal row was encoded from left to right (e.g. *W to the left of X*). Thus a top-to-bottom, left-to-right representation of the picture can account for the obtained interactions.

This form of representation can also account for the result that the *above–below* sentences were verified faster when they referred to the consonants in the left-hand column. This is entirely consistent with the hypothesis that, in the picture representation, the proposition about the left-hand column precedes the proposition about the right-hand column. The search through these propositions could start with the first, and terminate with the proposition containing the interrogated consonants. Then it would follow that test sentences that refer to the left side of the picture would be verified faster. Similarly, the *left–right* sentences were verified faster if they referred to the upper row of consonants than if they referred to the lower row. This is again consistent with the proposal that the proposition about the top of the picture precedes the proposition about the bottom. So a top-to-bottom, left-to-right representation of the consonants in the picture accounts for the position effect as well as for the true–false × unmarked–marked interaction described above.

The *above–below* asymmetry seems to be more general than the *right–left* asymmetry. *Above* has an advantage over *below* in two ways. Expt. II showed that when a description with *above* precedes a picture, the picture may be encoded faster than if the description is in terms of *below*. In addition, Expt. III showed that people spontaneously code the information in a picture so that it is congruent with the true description containing *above*. Therefore the advantage of *above* is consistent across the two tasks. However, the *right–left* asymmetry is not as consistent. Expt. II showed that when a description with *right* precedes a picture, then the picture is encoded faster than if the description is in terms of *left*. However, Expt. III showed that people spontaneously tended to code the information from the picture so that it is congruent with the true description containing *left*. This demonstrates that the *left–right* asymmetry is more task specific and it may even be culture specific.

The comparison between Expts. II and III reveals an interesting difference between verifying a sentence against a picture and verifying the same sentence against a long-term representation of that picture. Expt. III shows that different parts of the memorial representation of a picture are differentially accessible and the relative accessibility seems to be determined by the order of encoding. This is in marked contrast to the results for retrieval of information from an actual picture. Expt. II showed that different parts of the picture, left or right, top or bottom, were equally accessible. In this respect at least, mental representations of pictures are not like real pictures.

Even though the task encouraged the use of imagery (by asking about horizontal, vertical, and diagonal relations), and subjects were instructed to form images, and reported doing so, there is no independent evidence that they did use imagery. In fact, the results indicate that subjects used propositional representations of the simple pictures in performing this task.

GENERAL DISCUSSION

The concept of lexical marking has its beginnings in the field of linguistics. Bierwisch (1967) pointed out the consistent asymmetry between antonyms that refer to spatial dimensions, e.g. *high–low*. One of the adjectives, in this case *high*, is linguistically more basic or unmarked, while the other is more derived or marked. The unmarked term always refers to extent (e.g. *high, tall, deep*), while the marked term refers to lack of extent (e.g. *low, short, shallow*). One distinguishing property is that unmarked adjectives can be used to ask neutral questions (e.g. *How high is that flag? How tall is that man? How deep is that pool?*). These questions presuppose nothing about the relative extent of the object while questions with the marked adjective presuppose lack of extent (e.g. *How low is that flag? How short is that man? How shallow is that pool?*). In addition, the name of the adjectival dimension is sometimes morphologically related to the unmarked term (e.g. *height, depth*). but it is never morphologically related to the marked term. Bierwisch suggested that these properties of adjectives might be universal to all languages.

A linguistic universal like the marking of spatial adjectives may be based on powerful conceptual and environmental universals (Clark *et al.*, 1973). Therefore lexical marking should be manifested in a number of psychological processes. In fact, unmarked adjectives are processed faster and with fewer errors than marked ones in many tasks, such as deductive reasoning (Clark, 1969), sentence–picture verification (Just & Carpenter, 1971; Clark *et al.*, 1973) and sentence memory (Carpenter, 1974). In the sentence–picture verification task, the marking effect probably lies in one of the four stages that contribute to the total latency: encoding the word or sentence, encoding the picture, comparing the two representations, and outputting the result of the comparison process in a response.

The locus of the marking effect

The proposal that we advance attributes the advantage of *above* and *right* to the picture–encoding stage. In a situation where a sentence precedes a picture, the preposition of the sentence determines the nature of the encoding of the picture. The words *above* and *right* engender a more natural, canonical and therefore faster encoding. *Below* and *left* cause the picture to be encoded in a way that is not canonical and consumes more time. This encoding could take longer for one of two reasons. One possibility is that all relations are first encoded in a canonical way corresponding to the unmarked form. But if the preceding description contains a marked word, then the representation of the picture is recoded into the format that matches the marked form. Thus there would be an extra time-consuming recoding process involved in arriving at a picture representation that followed a marked description. The other possibility is that if the preceding verbal description is marked, then the picture is directly encoded in a format corresponding to the marked description, but it may take longer to derive this encoding. In either case, our proposal specifies that the picture-encoding process takes longer when a marked, rather than an unmarked, verbal description precedes the picture.

This model can account for the results of the current experiments, as well as a number of previous studies. Our sentence–picture verification task (Expt. II) showed the usual marking effect, although sentence encoding time was not included in the response latency. This is consistent with the proposal that the marking effect is located at the picture-encoding stage. In Expt. III, where the picture had been previously encoded (in fact, memorized), then the marking effect was abolished for *right–left*, and made unreliable for *above–below*. The main difference between Expts. II and III was that Expt. III did not include a picture-encoding stage. Otherwise the two experiments had the same pictures, sentences and responses. Since the marking effect was gone, or at least highly variable, it is reasonable to conclude that the effect is in the picture-encoding stage.

Although we have proposed that the marking effect occurs at the picture-encoding stage, a more general formulation would be that the marking effect occurs whenever a linguistically marked description determines the encoding of information from a second source, like a picture, semantic memory or an auditory display. This generalization seems sound because marking effects are found in tasks that do not involve pictures. For example, there are marking effects when the word *above* or *below* is verified with respect to the frequency of a test tone relative to a reference tone (Harvey, 1973). Also marking effects occur when sentences are verified with respect to one's knowledge of the world, e.g. *Giraffes are taller than goats* (Carpenter, 1974). Thus the effect of a marked description is to increase the time it takes to encode information from any second domain when the description governing that encoding is marked.

This generalization also accounts for the difference between semantic memory experiments that show a marking effect and Expt. III, where sentences were verified with respect to previously memorized pictures. In the semantic memory experiment cited above, with sentences like *Giraffes are taller than goats*, the information about the relative heights was probably computed after the sentence was read. Thus the sentence could influence the way the information was retrieved and coded. And there was a reliable marking effect. By contrast, in Expt. III the relative locations of the consonants in the picture were encoded before the sentence appeared. Thus the sentence had no opportunity to influence the encoding of the picture and there was no reliable marking effect. The marking effect occurs only when a description can influence the coding of its referent.

The influence of a description on the encoding of a subsequent picture has been investigated by monitoring subjects' eye fixations during a sentence–picture verification task (Carpenter & Just, 1972). Subjects were given sentences with unmarked quantifiers like *A majority* (*or large proportion*) *of the dots are red* or marked quantifiers like *A minority* (*or small proportion*)..., and a picture of a large and a small subset of dots. Following the word *large proportion* or *majority*, people fixated primarily the larger subset, while following *small proportion* or *minority* they fixated primarily the smaller subset. So marked and unmarked words made people look at different parts of the same picture in that study. This difference in locus of fixation can be interpreted as a difference in picture encoding. And the verification latencies in this task support the hypothesis that the encoding following a marked description takes extra time. Sentences with unmarked quantifiers were verified significantly faster than sentences with marked quantifiers (Just & Carpenter, 1971).

Two recent studies have shown that it is possible to control the picture encoding stage (and hence the marking difference) by means of explicit instructions on how to code the picture. Using stimuli like those in the eye-movement experiment described above, Just & Carpenter (1971) instructed subjects on whether to attend to the large or small subset of dots in the picture that preceded the sentence. When they were explicitly told to code the colour of the larger subset (and in a condition without explicit coding instructions) quantifiers that referred to the large subset (e.g. *a majority*, *a large proportion*) were verified faster. However, when subjects were explicitly instructed to encode the picture in terms of the colour of the smaller subset, then the sentences that referred to the small subset were verified faster. Uniformly imposing an unnatural encoding of the picture eliminated the usual marking effect.

In a similar study, Clark & Chase (1972) asked people to verify sentences like *The star is above the plus* with respect to previously presented pictures like $\overset{*}{+}$. When subjects were not instructed on how to encode the pictures, or when explicitly instructed to attend to the top of the picture, or to both parts of it, the advantage of *above* was between 93 and 117 msec. However, when subjects were instructed to attend to the bottom, the advantage dropped to 30 msec. In fact, for the true affirmative sentences, *above* was 33 msec. slower than *below*. These results are completely consistent with our proposal that *below* usually causes an unnatural and time-consuming coding of the picture. When people are explicitly instructed to use this unnatural encoding all the time, then the advantage of *above* diminishes or disappears.

A different class of explanation of the marking effect is offered by response availability models (Seymour, 1973, 1974), which attribute variation in response latencies to the ease or difficulty of translating from a particular word-picture display to a particular response (e.g. 'yes' or 'no'). Positive features in the representation of the word–picture display reduce the threshold for selection of a 'yes' response, which is selected by sampling semantic features until a threshold is exceeded (Seymour, 1973). The two studies that experimentally deal with response selection and allocation unfortunately provide contradictory results. Seymour (1974, expt. II) asked people to judge whether the position of a dot relative to a face was correctly described by the word *above* or *below*. When the face was inverted (i.e. mouth at the top, eyes at the bottom), the advantage of *above* disappeared, consistent with the response availability notion. However, in an analogous study where subjects judged whether the word *right* (or *left*) correctly described the position of a dot relative to a square *from the perspective of a person facing the subject*, the advantage of *right* over *left* persisted (Olson & Laxar, 1973). Both experiments changed the usual mapping from the physical display to the response, such that people respond positively ('yes') when there is a mismatch between the word and the physical position of the dot. The presence of the mismatch (a negative feature, according to Seymour, 1974) should have affected response availability similarly in the two studies. The inconsistency between the two results leaves the response availability model in some doubt.

SUMMARY

We have proposed that when a linguistic description contains a lexically marked term, then the process of encoding subsequent information from the referential domain takes longer. This view has direct implications for what makes a sentence or word 'hard' or 'easy' to process. According to this view, there are no hard or easy words or sentences *per se*. Rather, there are hard or easy correspondences between descriptions and their referents (Carpenter & Just, 1975). In general, the unmarked terms more closely correspond to the way we normally encode and conceptualize the environment.

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REFERENCES

BIERWISCH, M. (1967). Some semantic universals of German adjectivals. *Found. Lang.* **3**, 1–36.

BROOKS, L. (1967). The suppression of visualization by reading. *Q. J. exp. Psychol.* **19**, 289–299.

CARPENTER, P. A. (1974). On the comprehension, storage and retrieval of comparative sentences. *J. verb. Learn. verb. Behav.* **13**, 401–411.

CARPENTER, P. A. & JUST, M. A. (1972). Semantic control of eye movements during picture scanning in a sentence–picture verification task. *Percept. Psychophys.* **12**, 61–64.

CARPENTER, P. A. & JUST, M. A. (1975). Sentence comprehension: a psycholinguistic processing model of verification. *Psychol. Rev.* **82**, 45–73.

CHASE, W. G. & CLARK, H. H. (1971) Semantics in the perception of verticality. *Br. J. Psychol.* **62**, 311–326.

CLARK, H. H. (1969). Linguistic processes in deductive reasoning. *Psychol. Rev.* **76**, 387–404.

CLARK, H. H., CARPENTER, P. A. & JUST, M. A. (1973). On the meeting of semantics and perception. In W. G. Chase (ed.), *Visual Information Processing*. New York: Academic Press.

CLARK, H. H. & CHASE, W. G. (1972). On the process of comparing sentences against pictures. *Cogn. Psychol.* **3**, 472–517.

HARVEY, N. (1973). The semantics of high–low continua. *Br. J. Psychol.* **64**, 51–54.

JUST, M. A. (1974). Comprehending quantified sentences: the relation between sentence–picture and semantic memory verification. *Cogn. Psychol.* **6**, 216–236.

JUST, M. A. & CARPENTER, P. A. (1971). Comprehension of negation with quantification. *J. verbal Learn. verb. Behav.* **10**, 244–253.

OLSON, G. M. & LAXAR, K. (1973). Asymmetries in processing the terms 'right' and 'left'. *J. exp. Psychol.* **100**, 284–290.

POSNER, M. I. & MITCHELL, R. F. (1967). Chronometric analysis of classification. *Psychol. Rev.* **74**, 392–409.

SEYMOUR, P. H. K. (1969). Response latencies in judgements of spatial location. *Br. J. Psychol.* **60**, 31–39.

SEYMOUR, P. H. K. (1973). Judgements of verticality and response availability. *Bull. psychon. Soc.* **1**, 196–198.

SEYMOUR, P. H. K. (1974). Asymmetries in judgments of verticality. *J. exp. Psychol.* **102**, 447–455.

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CHOICE OF NON-REINFORCEMENT FOLLOWING FORCED ALTERNATION

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Previous research has suggested conditions for the learning of the non-rewarded instead of the rewarded of two alternatives. Two experiments were performed with a reconvergent maze, in which food reward depended upon passing through only one of the two pathways. Rats given a block of non-rewarded then a block of rewarded trials chose predominantly the rewarded pathway during free-choice extinction testing, but those subjected to simple alternation sequences of rewarded and non-rewarded trials chose primarily the non-rewarded pathway. Faster running on non-rewarded trials preceded choice of the non-rewarded pathway by alternation subjects, and the evidence suggests that increased training led to greater preference for the non-rewarded alternative.

It seems important to examine further a finding, reported and discussed earlier in this journal (Holding & Pritchatt, 1967, 1968; Sheldon, 1968), that mice in some conditions apparently learn the wrong (non-rewarded) choice. This result was obtained in a maze which consisted essentially of a runway interrupted by a central obstacle, with pathways diverging under or over the obstacle and reconverging in a final common path. Deriving predictions from Deutsch's (1960) postulates, Holding & Pritchatt (1967) showed that the behaviour of mice in this 'conditional' maze may result in two opposing outcomes. A food reward was made contingent upon passage over the obstacle, being withheld for passage underneath; forced-choice schedules during training were arranged by the manipulation of access doors. When training consisted of a block of non-reinforced trials followed by a block of reinforced trials the conventional result was obtained. However, when reinforced 'over' runs were alternated with non-reinforced 'under' runs the learning was apparently reversed, in that the predominant direction of choice was 'under'.

The behaviour of the animals was shown to differ significantly between the alternation schedule, an intermediate schedule and the block schedule, a result which was adequate for the Holding & Pritchatt (1967) predictions. It was not possible to draw the stronger conclusion that alternated animals learned significantly to make the wrong choice, since for other reasons all animals were reinforced in the direction opposite to their rather strong 'under' preference. In order to draw an unequivocal conclusion, the design of the present series of studies has been modified in several respects. Rats, rather than mice were used to eliminate the possibility of species-specific effects. The 'over–under' choice, with its strong tactile–kinaesthetic cue disparity, has been replaced by a right–left discrimination to yield considerably weaker spatial preferences, and different groups of animals were run for reinforcement in each direction to control for these preferences. Finally, alternation animals were run in two groups, in sequences ending either with a reinforced or with a non-reinforced trial.

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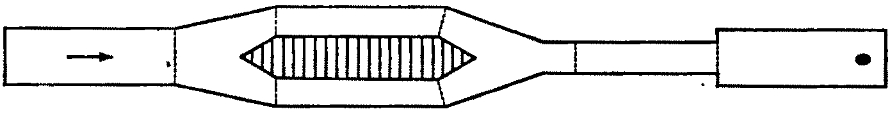

Fig. 1. Plan view of the reconvergent maze.

EXPERIMENT I

Method

Subjects and apparatus

Thirty-six male Sprague-Dawley rats completed the study; others were dropped as detailed below. Pre-experimental apparatus familiarization was given in a straightway consisting of two 6 × 6 in. start and goal boxes, linked by a 2 ft. 6 in. × $2\frac{1}{4}$ in. runway. The main experiment used a reconvergent maze, constructed with wooden walls and centre block 4 in. high and a wire mesh floor and top. The plan view and door locations are shown in Fig. 1, drawn to scale from the original length of 5 ft. 2 in.

Procedure

Animals were reduced to 80 per cent of normal *ad lib.* body weight during the week before testing and were subjected to 23 hr. deprivation before the main experiment. Water was freely available at all times in the home cages. On the afternoons preceding the main experiment, animals were given five reinforced runs in the straightway. This pretraining was intended both as a familiarization exercise and as a screening device; in all, three animals were discarded for not running at this stage. In both the pretraining and training procedures animals spent 30 sec. in the goalbox and 1 min. in a holding cage between runs. Reward consisted of three 45 mg Noyes pellets.

Training schedules in the reconvergent maze were arranged by opening or closing the appropriate doors at the choice point. For half the animals, reward was available upon traversing the left-hand pathway; for the other half, the right-hand pathway was rewarded. For the block group of 12 animals, training consisted of eight non-rewarded followed by eight rewarded trials. For the two alternation groups, each with 12 animals, there were eight trials to the rewarded side alternating with eight trials to the non-rewarded side. Alternation group NF began training with a non-rewarded trial (ended with a rewarded trial), and group FN began with a rewarded trial.

After the training trials, all animals received five non-rewarded test trials with both pathways open. Choice of rewarded or non-rewarded pathway on each trial was recorded, as was each start-to-goalbox latency. The five animals who 'froze' in the start box or alleys for periods exceeding 15 min. (three during training and two during testing) were replaced.

Results

Choice

The number of choices made in a given direction remained almost constant over successive test trials within all three groups. The results have therefore been expressed in Table 1 in terms of the number of animals making the indicated number of choices in the previously non-reinforced direction.

For significance testing, comparisons were based on the number of animals making three or more of their choices in one or the other direction. The block animals predominantly chose the rewarded pathway, as expected, with 10 out of 12 animals taking that direction; the result is significant ($P < 0{\cdot}02$, one-tailed) by a binomial test made against the chance expectation of random choice. Alternation group NF, which ended training with a rewarded trial, had 7 out of 12 animals choosing the

Table 1. *Number of animals making choices in non-rewarded direction (Expt. I)*

Group	Number of choices 0	1	2	3	4	5
Block	5	4	1	1	1	0
Alt. NF	2	2	1	4	3	0
Alt. FN	1	0	1	3	6	1

previously *non-rewarded* direction (non-significant) while group FN, which ended with a non-rewarded trial, had 10 out of 12 choosing the non-rewarded direction ($P < 0{\cdot}02$, tested as above).

Latency

Running times during training seemed to show a pattern similar to the results of the choice data. Disregarding the first rewarded and non-rewarded trials, which produced long and highly variable latencies, 11 of the 12 block animals had shorter median latencies on rewarded training trials (binomial; $P < 0{\cdot}01$, two-tailed). Similarly, 6 of the 12 group NF animals (not significant), and 10 of 11 group FN animals showing a difference had lower median latencies on the *non-rewarded* trials ($P < 0{\cdot}02$). The two alternation groups did not differ significantly.

Examining the data for the two alternation groups together, 13 out of the 16 who showed faster median running times on non-rewarded trials also showed a preference for the non-rewarded pathway during testing ($P < 0{\cdot}02$); only three out of the seven who showed faster times on rewarded trials chose the non-rewarded alley. No effect due to rewarding the right or left pathway appeared.

EXPERIMENT II

Although the results of Expt. I support the previous study, the data are somewhat variable. A definite conclusion, that rats forced to alternate between rewarded and non-rewarded pathways later choose the non-rewarded pathway, seems to require broader support. Further work with 29 pilot animals, each given a reduced total of eight training trials, did not substantiate the apparent difference between the NF and FN treatments. However, the reduction in training time lowered the proportion of non-rewarded choices to borderline significance (rewarded side, 10; non-rewarded side, 19 animals). Expt. II was therefore designed to examine in greater detail the effects of varying the amount of training.

Method

Ten additional male Sprague-Dawley rats were randomly assigned in groups of five each to treatments NF and FN, receiving pretraining, training and testing as in Expt. I. However, at least one day after completion of testing, they again received eight training and five test trials; then, at least a day later, they received a final eight training and five test trials. No rats were rejected from this study for any reason.

Table 2. *Number of animals making choices in the non-rewarded direction on successive blocks of trials (Expt. II)*

	Number of choices					
Session	0	1	2	3	4	5
1	3	0	3	4	0	0
2	0	1	2	5	1	1
3	0	1	1	5	2	1

Results

Choice

The two alternation groups did not differ, and are considered together. Table 2 shows the numbers of choices at the three stages of training. There is an apparent increase in choice of the non-rewarded side as a function of training, the final proportion of 8 out of 10 giving a (one-tailed) binomial probability of 0·055. The learning effect may be evaluated by considering the six animals who chose the rewarded pathway during the first session; four shifted to the non-rewarded path in the second session, and all six chose it in the third session. This is a significant shift from the first to third sessions ($P < 0{\cdot}05$, two-tailed binomial). Only two out of the four rats choosing the non-rewarded side during the first session later switched to the rewarded side, the remaining two consistently choosing the non-rewarded side in all three sessions.

Latency

The latency data are again consistent with the previous two studies. Of eight rats running faster on non-rewarded trials in the last session (again discounting the first run of each kind), seven chose primarily the non-rewarded alley ($P < 0{\cdot}05$, one-tailed). As in the earlier data, animals showing lower latencies on the rewarded trials split; one preferred the rewarded, and one the non-rewarded, alley.

DISCUSSION

The two experiments, taken together, seem to show clearly that forced alternation results in preference for the non-rewarded pathway. Reinforced for turning right, these animals predominantly choose to turn left; reinforced for left, they predominantly turn right. In all, 25 of the 34 rats in the alternation groups finished by choosing the non-rewarded side on the majority of their test trials. This proportion is not only significantly different from the choice scores of the block animals, but is clearly significant ($P < 0{\cdot}01$, two-tailed binomial) against the equal proportions expected with random choice. Further, the data from Expt. II seem to indicate that the effect is lawful, since the tendency to choose the non-rewarded side increases as a function of the number of training trials.

These experiments are difficult to compare with the existing literature, as the design bears similarities, often superficial, to many other areas of investigation. The shape of the maze is ambiguous, and it is not known how far the findings may be generalized to other designs. To the extent that the alternation of reward and non-reward in the reconvergent maze may be likened to reward alternation in a single alley, the data might be expected to show the sequential patterning effects described by Capaldi (1967). In these single alley experiments the dependent variable is running time, which becomes shorter on reinforced and longer on non-reinforced runs as learning progresses. Amsel's (1967) frustration theory would also predict faster running on reinforced trials since, under alternation, these are the trials which follow non-reinforcement. The present experiments show the reverse effect, each study yielding lower median latencies for the non-rewarded pathway. Furthermore, the present running speeds predict the direction of choice. It may be, therefore, that rats tend to choose the alternative in which they have run faster. In that case, it becomes relevant that the Capaldi (1967) type of patterning does not become stabilized without extended practice over many days of training.

It has been shown (Tyler *et al.*, 1953) that reversed running times (fast on non-rewarded, slow on rewarded) become established during the first 30 or so trials of single alley alternation. The pattern then crosses over, with rewarded runs becoming faster than non-rewarded. On later extinction trials, alternated animals extinguish faster (run slower) than randomly reinforced animals. If one assumes that, later in training, the after-effects of non-reinforcement become conditioned to running faster on the next trial, and vice versa, it is impossible to explain the extinction data. However, if one assumes instead that the consequences of reinforcement or non-reinforcement directly impinge on running speeds (but are overlaid by a secondary process during later trials), one can explain both the early trial data and the extinction effects. The necessary assumption is simply that the direct after-effect of reinforcement, apart from secondary associative and perhaps frustration effects, is to increase running speed, and the direct after-effect of non-reinforcement is slower running.

The same assumption is applicable to the present data, where it constitutes a statement of the results. Following a non-reinforced trial, animals ran slower on the next trial, which the alternation schedule designated as a reinforced trial. Following a reinforced trial, animals ran faster on the next, non-reinforced trial (and eventually chose the faster run, non-reinforced pathway). The data suggest, therefore, that early in learning the forward-acting effects of reinforcement may be more important than the traditionally emphasized retroactive effects.

References

Amsel, A. (1967). Partial reinforcement effects on vigour and persistence. In K. W. Spence & J. T. Spence (eds.), *The Psychology of Learning and Motivation*. New York: Academic Press.

Capaldi, E. J. (1967). A sequential hypothesis of instrumental learning. In K. W. Spence & J. T. Spence (eds.), *The Psychology of Learning and Motivation*. New York: Academic Press.

Deutsch, J. A. (1960). *The Structural Basis of Behaviour*. London: Cambridge University Press.

Holding, D. H. & Pritchatt, D. (1967). Deutsch's model applied to a conditional maze. *Br. J. Psychol.* **58**, 85–92.

Holding, D. H. & Pritchatt, D. (1968). Reply to Sheldon. *Br. J. Psychol.* **59**, 452.

Sheldon, M. H. (1968). Deutsch's model applied to a conditional maze. *Br. J. Psychol.* **59**, 449–452.

Tyler, D. W., Wortz, E. C. & Bitterman, M. E. (1953). The effect of random and alternating partial reinforcement on resistance to extinction in the rat. *Am. J. Psychol.* **66**, 37–65.

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SPATIAL MEMORY BY BLIND AND SIGHTED CHILDREN

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Non-verbal recall of haptically presented spatial positions by three age groups of blind and sighted children was tested under conditions varying cueing, recall type and stimulus position in a within-subject design. Sighted status was not only significant, but interacted significantly with recall type, and further with stimulus position, consistent with sequential haptic by blind and quasi-simultaneous visual processing by sighted children. Age was significant, but its only significant interaction was a relatively small one with cueing conditions and stimulus position, suggesting that the oldest group, regardless of sightedness, used verbal strategies in pre-cued conditions. The findings support the hypothesis that visual and haptic modalities of representation have demonstrably different effects on processing and efficiency in spatial recall, but counterindicate the hypothesis that these relate differentially to age. Results also suggest that a combination of cue utilization and verbal strategies is a significant, but relatively minor, factor in improvements in spatial recall.

The nature and function of imagery in memory for spatial position, the extent to which children use it, and its relation to other factors which affect memory are by no means clear. At least three views are possible; Piaget & Inhelder (1971, 1973) regard visual imagery as largely irrelevant, and stress the level of abstraction from actual action by the subject in the development of spatial as of other types of memory. This may mean either that modalities of representation have no differential effects and serve as 'props' for mental operations in the same way, or that action and movement with regard to objects and positions, and thus haptic factors, have special relations to memory functions. A second view is that visual imagery has specifically facilitating effects in spatial memory. Huttenlocher & Presson (1973) suggest that children, like adults (Attneave & Benson, 1969; Rock, 1974; Shephard & Feng, 1972; Shephard & Metzler, 1971), visually image spatial layouts and mentally track positions on it. This suggests that visual imagery has a special role in spatial memory and possibly also in improvements with age. A third possible view is that spatial memory and development depend on verbal strategies.

Theoretically, these views involve questions about 'memory modalities' or, more simply, whether differential modality effects can be demonstrated in spatial memory, and, if so, how these relate to cognitive skills; for instance, the organization of material for recall, and the utilization of prior information; and to age. Memory for haptic (touch and movement) inputs, about which relatively little is known as yet, is of particular interest, because it provides a test case for the nature and effects of representations, but also because it is of practical importance in teaching the blind who need to rely on spatial memory far more than the sighted.

The present study was therefore designed to test hypotheses arising from these differing views. Although visual imagery is frequently reported and suggested as an explanation, it is more difficult to find experimentally demonstrated differential effects on spatial memory in children. The methodological problems and difficulties of interpretation inherent in Piagetian and Paivio-type methods have been discussed by Brainerd & Heuvel (1974) and do not require repetition. Their own findings depend on differences in training with 'reinforced discrimination', or imitative or anticipatory drawing for recognizing and drawing two-dimensional plans of three-dimensional geometric objects. However, all conditions involved vision. The advantage found for 'motoric' (drawing) conditions could therefore have been due to visual feedback, increased drawing skill or information added by drawing and by 'reinforcing' already available discriminations, rather than to differences in encoding. Neither age effects nor correlations with scores on cognitive tasks can therefore be interpreted univocally. O'Connor & Hermelin (1972) showed the importance of modes of presentation: deaf children reported the spatially middle digit, blind children the sequentially middle digit, and normal controls the spatially or sequentially middle according to whether presentation was visual or auditory. Indeed, sighted children do not necessarily spontaneously recode haptic inputs visually with verbal (Hermelin & O'Connor, 1971) or recognition (Millar, 1974) tests. But this may depend on the type of test and material. A current study by the author on mental spatial rotation of haptically presented spatial inputs found differences between the blind and sighted with recall but not with recognition tests.

The question whether there are demonstrable differences in habitual modes of representation depends on whether there are differences in strategy when inputs are the same. Thus, for adults, spatial images are probably not literal copies (Neisser & Kerr, 1973). Nevertheless, visual perception and visual imagery are close enough to interfere (Brooks, 1967; Atwood, 1971). It may therefore be suggested that the simultaneity which characterizes visual perception carries over to visual representations. For spatial recall this may have distinct advantages. Thus, if it is possible to keep in mind, or to evoke, several positions simultaneously or in parallel, backward recall – i.e. recall of positions from the end of inputs – should be no more difficult than forward recall, since reorganization would be redundant. Conversely, if the sequential nature of haptic as of verbal inputs carries over to representation in memory, backward recall which requires reorganization should be more difficult. This provides a test for the hypothesis that sighted children use visual imagery in spatial recall. It would be supported if sighted children were not only better at non-verbal recall of haptic inputs than the early or congenitally blind who can have little or no such imagery, but more importantly, if sightedness interacted with recall type. No such difference would be expected if visual representations are not used, or are irrelevant to spatial recall.

The hypothesis that spatial inputs are coded verbally would not predict differences between the blind and sighted either. However, verbal strategies should produce some differential effects. Thus older children may be expected to use them more than younger ones (Conrad, 1971). Bernbach (1967) showed that the primacy effect, absent in serial recall by young children, could be produced by teaching them to verbally label the items. Consequently, on the assumption that the primacy effect is mainly due to longer-term verbal encoding and rehearsal (Atkinson & Shiffrin, 1968), a differential age effect for positions near the start (primary) of a sequentially presented spatial display should be found if verbal strategies are used.

Modality-specific haptic encoding of non-spatial tactual inputs has been found in several recent studies (Miller, 1975*a*, *b*), but seems to be less efficient, and less subject to active rehearsal than verbal memory (Sullivan & Turvey, 1972), either early in delay (Gilson & Baddeley, 1969) or with unfamiliar non-verbal material (Millar, 1974). In the author's current study on mental spatial rotation the sighted seemed to rely on visual strategies from the point of stoppage, while the blind were differentially penalized on the more complex directions and lost track already after two turns, consistent with sequential coding and the findings that haptic memory is less subject to active strategies, except with familiar material. Thus, if the blind rely on haptic and movement rather than on verbal coding for spatial recall, a differential effect on the primacy position for the older blind should not be found, while an interaction of recall type with sighted status would be predicted.

Support for the hypothesis that modality of representation is a factor in development with age would be shown by appropriate age interactions. Rate of forgetting as such does not seem to be an important factor in memory development (Belmont & Butterfield, 1969). It is of interest, however, to relate the role, if any, of the modality of spatial representation to factors involving mental work in memory: the reorganization of material as in backward recall, and the utilization of prior information for encoding.

This study therefore tested spatial recall by three age groups of blind and sighted children, varying cueing conditions, type of recall and stimulus position to test the hypotheses that (1) the modality of representation has differential effects on efficiency and on strategies of spatial recall; (2) this is a factor in improvements with age; and (3) improvements with age in spatial recall depend upon verbal strategies, organization, or cue utilization.

METHOD

A five-way factorial design with age, sighted status, pre- and post-cueing, recall type, and stimulus position (repeated measures on the last three factors) was used for non-verbal recall of haptically presented spatial positions as follows.

Subjects

There were 72 subjects: 36 congenitally or very early (20 months or less) totally or severely blind (minimal light, but no pattern perception) from two residential schools for the blind; and 36 sighted children from two local authority schools within the catchment area, and similar in range of ability (low average to very superior) and socio-economic status of the parents. The sighted were matched to the blind on sex, age (within three calendar months), forward digit span, and backward digit span (ascertained in the manner of Binet tests). IQ scores, although necessarily on different tests, available for the blind and some of the sighted, and ability ratings by teachers were taken into account in matching.

There were 12 sighted and 12 blind subjects in each of three age groups – oldest groups: seven girls, five boys; mean age 10:4 (from 9:1 to 11:8); mean forward span, 6·0 (from 5 to 8); mean backward span, 3·2 (from 2 to 4); middle groups: four girls, eight boys; mean age, 8:2 (from 7:0 to 8:10); mean forward span, 5·4 (from 4 to 7); mean backward span, 2·9 (from 0 to 5); youngest groups: six girls, six boys; mean age, 6:11 (from 6:2 to 7:5); mean forward span, 5·0 (from 4 to 7); mean backward span, 2·2 (from 0 to 4).

Materials

The material consisted of a 35 × 4 cm slot cut in a 20·25 cm Perspex square to form a five-sided open-shaped spatial plan with four right-angled turns. This was presented in one of four positions (north, south, east, west) in randomized order across conditions and trials for each subject.

For presentation, the subject traced along the slot with a pencil from the start to prepared stops (stimulus positions), and then on to the end of the slot. The stops were formed by the suction-base of a 10 cm tall plastic stick placed across the slot so that it arrested the pencil and the subject's hand at this point. It was removed after contact and the subject continued tracing the slot to the end. The recall task was to trace the slot with the pencil, either from the start or from the end of the slot, to the position on the slot where the stop had been encountered on the presentation trial. After recall the stop was replaced for the next presentation.

Subjects used only their preferred hand to hold the pencil. They were not permitted to palpate the display except by tracing through the slot with the pencil in the experimental conditions. For sighted subjects the display was presented in a curtained unit which prevented viewing, but permitted free hand and arm movements.

Task

The task was presented as a game of teaching 'Bozo' – a rubber animal fastened to the end of the pencil – to remember the position of a traffic-light post, so that he would stop at that position on the road next time.

The starting point for each presentation was the start of the slot nearest to the subject and his preferred hand, in one of the four positions. This point was called the subjects' 'house'. The end-point of the slot was called 'Bozo's kennel', and the slot was 'the road between the subject's house and Bozo's kennel'. The stop formed by the stick and suction base was called the 'traffic-light post'.

The subject was told that he always had to take Bozo on two journeys (by tracing the slot with the pencil to which Bozo was fastened) between his house and Bozo's kennel. The first journey would always be from the subject's 'house' (starting-point) to Bozo's kennel (end-point), but they would be stopped by colliding with a traffic light-post somewhere on the road. The post would then be removed so that they could go on to Bozo's kennel. The main point was to remember the point on the road where they had felt the post. On the second journey the traffic-light post would not be there. Nevertheless, on the second journey Bozo would have to stop dead at the point where the traffic-light post had been on the first journey ('to avoid traffic hazards'). Presentation consisted in the 'first journey' from house to kennel. The recall task was the 'second journey'.

Type of recall

Type of recall was either forward or backward. *Forward recall*: the subject was instructed that the second journey between the 'house' (starting-point) and the 'kennel' (end-point) would start again at the same point – the subject's 'house'. He would have to take Bozo along the road from the house until he came to the point where he and Bozo had felt the traffic-light post on the first journey. This meant that he would have to trace through the slot with the pencil to which Bozo was fastened to that point and not any farther. The traffic post would no longer be there, but he had to make sure that Bozo stopped at exactly the same point on the road where the traffic-light post had been on the first journey. *Backward recall*: the subject was instructed that the second journey between the 'house' and the 'kennel' would start from the 'kennel' at the end of the road. He would have to take Bozo back along the road (trace through the slot, etc.) from the kennel (end-point of the slot) to the point on the road where they had felt the traffic-light post on the first journey. The traffic post would no longer be there, but he had to make sure that Bozo would stop at exactly that point.

Recall was immediate (approx. 3 sec.) after the end of presentation, and was signalled by 'same, from the house' or 'back, from kennel', as appropriate for forward and backward recall. To ensure a rapid start to the recall, the experimenter helped the subject to insert the top of the pencil at the start or end of the slot, as appropriate.

Cueing conditions

There were two cueing conditions. They differed solely on whether instructions for the type of recall were given prior to presentation (first journey) or only prior to recall (second journey). *Pre-cued conditions*: the subject was informed prior to a block of forward recall runs that recall (the second journey) would always start at the same point – his 'house' – on the second journey as on the first. For the block of backward recall runs, the subject was informed prior to the block of runs that recall (second journeys) would always be back from the 'kennel' (end-point of slot). The instructions were repeated prior to each presentation ('Remember, the second journey will

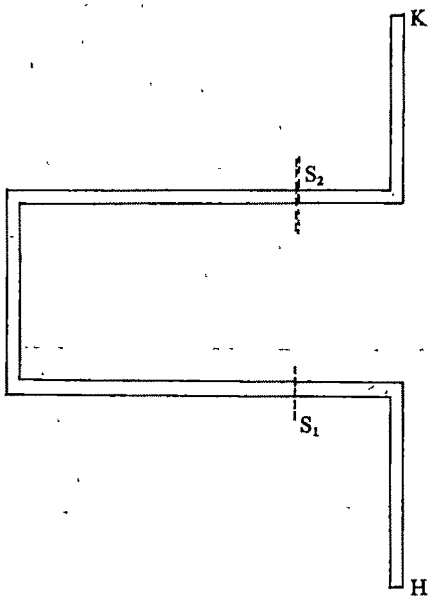

Fig. 1. The five-sided spatial display slot here shown in the 'West' position. Input during presentation is from tracing the slot from H (house) to K (kennel). S_1 represents the first (near), S_2 represent the recent (far) stimulus position. Forward recall is from H to either S_1 or S_2. Backward recall is from K to either S_1 or S_2.

also start from this house' or 'Remember, the second journey will start from the kennel at the end'). Presentation was then signalled immediately by 'start'. *Post-cued conditions*: the subject was told prior to the whole block of post-cued runs that the second journeys could either be forward from the house, starting at the same point as the first journey or backward from the kennel, starting back from the end-point, but that he would not know before each of the first journeys whether the second one would be from the house or from the kennel. He would, however, be told which of these it was to be, at the start of each of the second journeys by the signal 'same, from the house' or 'back, from kennel'. Presentation was signalled by 'start'.

Subjects had no difficulty in understanding the task, and thoroughly enjoyed the 'game'.

Stimulus position

Two positions on the display, one near the starting point (7·5 cm) and one far from it (27·5 cm), were tested. Since tracing the display was sequential, the near stimulus represented an initial, and the far stimulus a recent, stimulus in relation to the touch and movement inputs of the display. The display slot, start ('house') and end ('kennel') of presentation, and the near (first) and far (recent) stimulus positions are shown in Fig. 1.

Procedure

Subjects were tested singly in a quiet room of their school. The game was explained to them, and they received two demonstration trials (not counted), followed by 16 main trials (four in each of the conditions: pre-cued forward, pre-cued backward, post-cued forward, post-cued backward). The order of cueing conditions, blocked in threes (post-cueing both recall types in counterbalanced order, pre-cued forward; pre-cued backward recall), recall type and stimulus position were counterbalanced across subjects.

RESULTS AND DISCUSSION

Errors were counted in mm deviations from the stimulus position. Mean absolute errors, shown in Table 1, were subjected to a five-way ANOVA with age (youngest, middle, oldest), sighted status (blind, sighted), cueing conditions (pre–post), type of recall (forward, backward) and stimulus position (near, far) as variables (repeated measures on the last three factors).

Table 1. *Mean absolute errors (mm) for blind and sighted children under different cueing, recall and stimulus position conditions*

		Pre-cueing				Post-cueing			
		Forward recall		Backward recall		Forward recall		Backward recall	
Stimulus position ...		Near	Far	Near	Far	Near	Far	Near	Far
Mean age									
Blind	6:11	69	70	100	59	97	58	103	55
	8:2	58	67	119	37	62	76	119	54
	10:4	51	40	90	42	85	54	96	54
Mean		59	59	103	46	81	63	106	54
Sighted	6:11	86	52	78	41	101	70	66	69
	8:2	110	51	57	40	47	40	41	62
	10:4	31	21	44	21	60	22	41	11
Mean		76	41	60	34	73	44	49	47

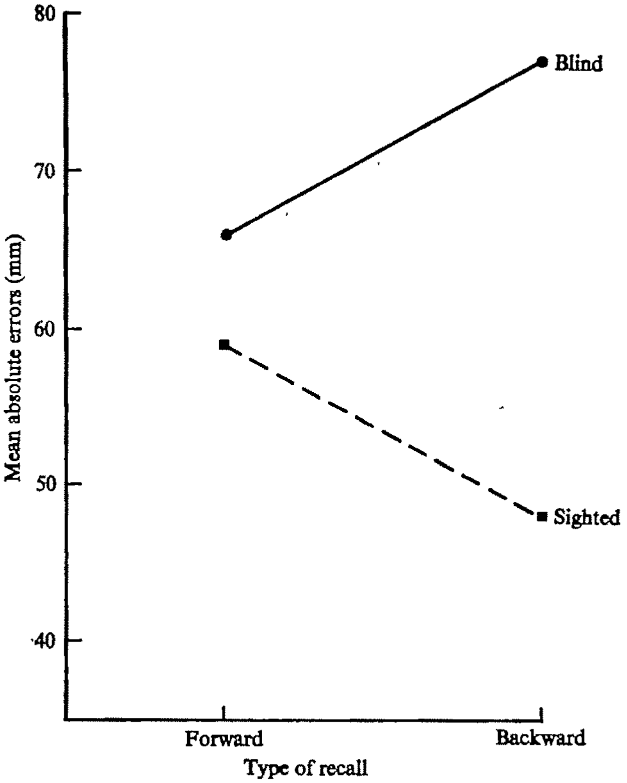

Fig. 2. Mean absolute errors (mm) for forward and backward recall by blind and sighted children.

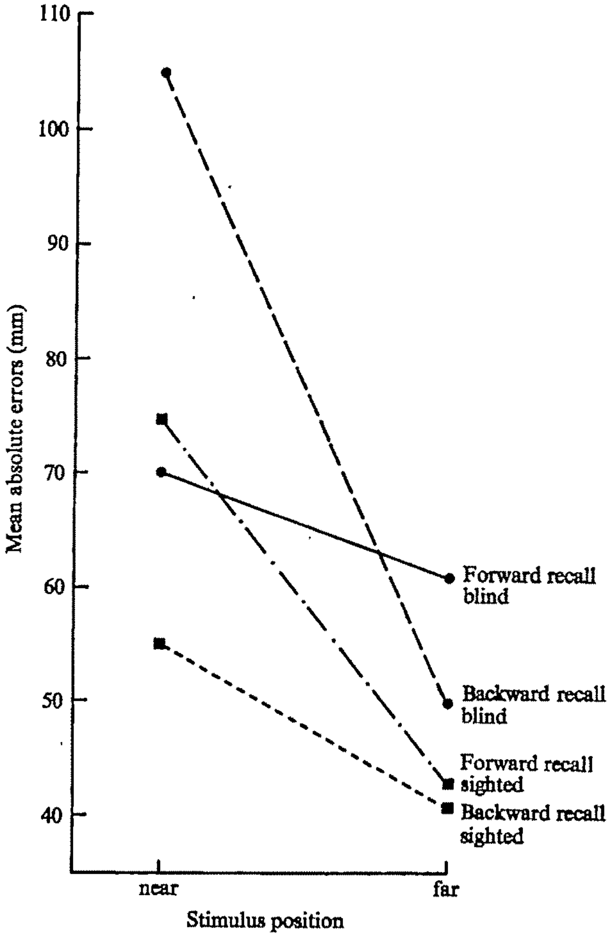

Fig. 3. Mean absolute errors (mm) by blind and sighted children for near and far stimuli in forward and backward recall.

Sighted status was a significant effect ($F = 12{\cdot}04$; d.f. = 1, 66; $P < 0{\cdot}001$) and interacted significantly with recall type ($F = 10{\cdot}0$; d.f. = 1, 66; $P < 0{\cdot}005$), supporting the first hypothesis. The interaction, shown in Fig. 2, shows not only that the blind produced larger errors, but that the main difference was obviously on backward recall, with significantly ($P < 0{\cdot}01$) worse backward than forward recall for the blind, while for the sighted the relation was reversed. This supports the assumption that the blind used sequential strategies and the sighted did not.

The relation of sighted status to recall type was further elucidated by the fact that this interacted also with stimulus position ($F = 11{\cdot}40$; d.f. = 1, 66; $P < 0{\cdot}005$). Stimulus position was indeed a highly significant main effect ($F = 69{\cdot}97$; d.f. = 1, 66; $P < 0{\cdot}0001$). This meant that the near (primacy) position was very much more difficult. The interaction of sighted status, recall type and stimulus position is graphed in Fig. 3. This shows that for the blind the difference between recall of the two positions was clearly greater for backward recall, with greatly increased errors on the near (primacy) position, and some facilitation on the recent one. Since distance and time to recall from stimulus presentation to recall point were greatest for the near position and least for the far position for backward recall, and between these for forward recall, this implies that time and distance determined errors by the blind. This suggests a sequential haptic rather than a verbal rehearsal strategy. For the sighted, the picture is very different. The recent position was unaffected by recall type, but responses for the near (primacy) position were actually easier in backward than forward recall, i.e. in the opposite direction from what would be predicted with verbal strategies. Reorganization of sequential verbally encoded positions on the display would produce larger rather than smaller error for backward recall, and verbal rehearsal would not penalize responses to the near position in forward recall. A completely simultaneous strategy could not have been used either. The results suggest that one sighted visually evoked or envisaged the display from the end-point (at recall) regardless of cueing conditions, rather than during presentation. This would penalize responses more on the near position in forward recall. A similar strategy of reconstructing from the point of stoppage was used by the sighted in the author's current study on mental rotation of a haptically presented line.

Age ($F = 8{\cdot}23$; d.f. = 2, 66; $P < 0{\cdot}001$) was significant, but did not significantly interact with sighted status in any combination, counterindicating the hypothesis that modality effects were a determining factor in improvements with age.

The only significant interaction of age with other factors was with a combination of cueing conditions and stimulus position ($F = 3{\cdot}25$; d.f. = 2, 66; $P < 0{\cdot}05$), graphed in Fig. 4. This shows that pre-cueing significantly ($P < 0{\cdot}05$) facilitated responses for the near (primacy), but not for the far (recency) position for the oldest children, regardless of sighted status, suggesting that they used verbal strategies. For the 8-year-olds, on the other hand, pre-cueing actually had detrimental effects on the near position, while for the youngest group pre-cueing did not significantly differ from post-cueing and did not affect stimulus position, suggesting that these groups did not use verbal encoding and rehearsal which would be expected to facilitate recall specifically for the first position with prior information. The fact that neither cueing conditions nor stimulus position alone related to age, except in conjunction with each other, suggests that utilizing prior information may have detrimental effects (as on the middle group) for the more difficult position unless it elicits verbal strategies (as by the oldest group), or if prior information is not utilized significantly (as by the youngest group). The interaction was, however, relatively small, and would not account for the whole of the age variance.

GENERAL DISCUSSION AND SUMMARY

The main results were clear. The significant interaction between sighted status and recall type was predicted on the hypothesis that the blind and sighted used different strategies. This, the further interaction of these variables with stimulus position, the main effects of sighted status and of stimulus position were all consistent with the assumption that the blind relied on haptic memory which decays with time or is interfered with by subsequent movements rather than on verbal coding and rehearsal; and that the sighted used visual representations which did not penalize backward recall. They apparently relied on envisaging the display and positions from the end-

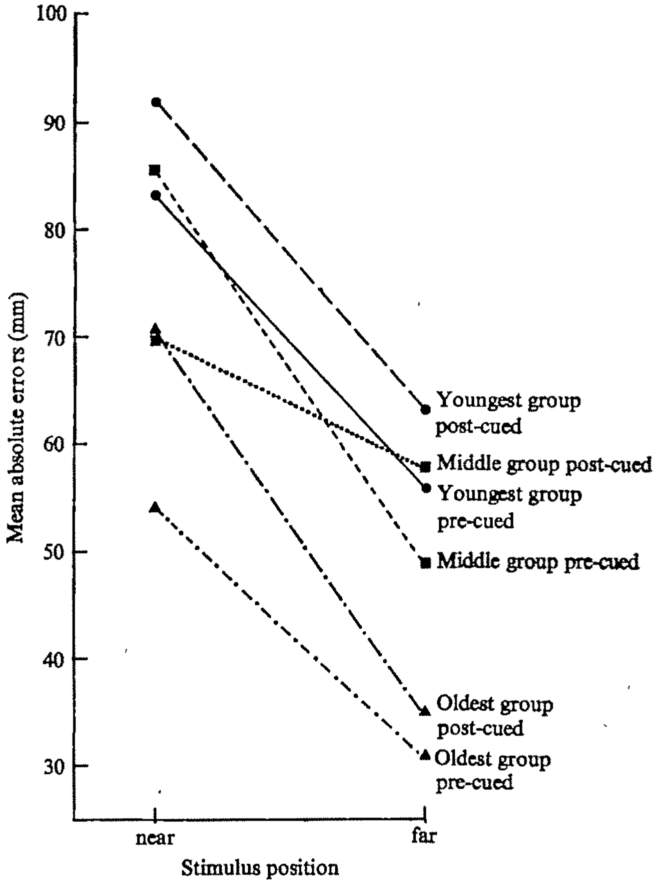

Fig. 4. Mean absolute errors (mm) for the youngest (mean age, 6:11), middle (mean age, 8:2) and oldest (mean age, 10:4) groups for near and far stimuli in pre- and post-cued conditions.

point, possibly because they mainly recoded visually at recall rather than during presentation. The findings supported the hypothesis that modalities of representation differentially affect efficiency and processing in spatial recall.

The findings also supported the view that visual imagery has a special role in spatial recall by children. In so far as Piaget's theory implies the opposite, or suggests that visual imagery provides 'props' in the same way as other types of representation, this was counterindicated. The absence of any interactions between sighted status and age, also counterindicated the implication in Piaget's theory that haptic and movement representations have a special role in age changes. At the same time, this finding does support his view that visual imagery as such does not relate to age. It suggests indeed that age changes were independent of visual and haptic modality-specific processing.

The fact that recall type did not relate to age may explain this. For visual – simultaneous or evoked imagery – reorganization and therefore worse backward recall and age changes in this with increased use of active organizational strategies would be irrelevant and was not found for the sighted. A difference penalizing backward recall was predicted for sequential haptic processing and was found for the blind. The fact that this did not interact with age either is consistent with previous findings that haptic memory is less subject to active strategies unless the material is over-learned (Gilson & Baddeley, 1969; Millar, 1974, 1975*a*, *b*; Sullivan & Turvey, 1972). A reasonable explanation of the absence of relations between modality effects and age is therefore that visual imagery and processing does not require, and haptic representations and processing do not readily lend themselves to, active reorganization.

Finally, the fact that the only variables which interacted significantly with age were a combination of cueing conditions and stimulus position which implied that utilization of prior information elicited verbal strategies by both blind and sighted subjects in the oldest group, must be considered. Since the effect, although significant, was relatively small, and neither cueing conditions nor stimulus position alone related to age, this suggests (i) that age changes depended on combinations of factors rather than on any single cognitive or verbal one, and (ii) that cue utilization and verbal coding played a significant, but relatively small part in age changes in spatial memory.

The practical implications for the blind are twofold. First, the deliberate use of verbal labelling and rehearsal, particularly at the time spatial positions are first encountered, seems to aid recall of the more difficult positions, but probably requires specific teaching and encouragement in younger children who do not seem to do this spontaneously. Secondly, since the effect was small and the blind apparently mainly relied on memory for touch and movement, repeated actual familiarization with spatial layouts which allows over-learning of touch and movement cues is probably essential.

In summary, the findings suggest a model of spatial memory in children which includes modalities of representation as important factors, determining type of processing, but not changes with age. It was argued that this may be because visual and haptic, or modality-specific representations and the type of processing these elicit do not easily lend themselves to active controls. The findings suggest further that such a model may need to include changes to a combination of verbal strategies and utilization of prior information for encoding as a significant, but relatively minor factor in age changes in spatial memory. The main improvements in spatial memory with age seemed to take place within the modalities of representation without being determined by the differences between them.

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REFERENCES

ATKINSON, R. C. & SHIFFRIN, R. M. (1968). Human memory: a proposed system and its control processes. In K. W. Spence & J. T. Spence (eds.), *The Psychology of Learning and Motivation*, vol. 2. New York: Academic Press.

ATTNEAVE, F. & BENSON, B. (1969). Spatial coding of tactual stimulation. *J. exp. Psychol.* **81**, 216–222.

ATWOOD, G. (1971). An experimental study of visual imagination and memory. *Cogn. Psychol.* **2**, 290–299.

BELMONT, Y. M. & BUTTERFIELD, E. C. (1969). The relations of short-term memory to development and intelligence. In L. P. Lipsitt & H. W. Reese (eds.), *Advances in Child Development and Behaviour*. London: Academic Press.

BERNBACH, H. A. (1967). The effect of labels in short term memory for colours with nursery school children. *Psychon. Sci.* **7**, 149–150.

BRAINERD, C. J. & HEUVEL, K. V. (1974). Development of geometric imagery in five- to eight-year olds. *Genet. Psychol. Monogr.* **89**, 89–143.

BROOKS, L. R. (1967). The suppression of visualizing by reading. *Q. J. exp. Psychol.* **19**, 289–299.

CONRAD, R. (1971). The chronology of the development of covert speech in children. *Dev. Psychol.* **5**, 398–405.

GILSON, E. Q. & BADDELEY, A. D. (1969). Tactile short-term memory. *Q. J. exp. Psychol.* **21**, 180–184.

HERMELIN, B. & O'CONNOR, N. (1971). Spatial coding in normal, autistic and blind children. *Percept. mot. Skills* **33**, 127–132.

HUTTENLOCHER, J. & PRESSON, C. C. (1973). Mental rotation and the perspective problem. *Cogn. Psychol.* **4**, 277–299.

MILLAR, S. (1974). Tactile short-term memory by blind and sighted children. *Br. J. Psychol.* **65**, 253–263.

MILLAR, S. (1975*a*). Effects of phonological and tactual similarity on serial object recall by blind and sighted children. *Cortex* (in press).

MILLAR, S. (1975*b*). Effects of tactual and phonological similarity on the recall of Braille letters by blind children. *Br. J. Psychol.* **66**, 193–201.

NEISSER, V. & KERR, N. (1973). Spatial and mnemonic properties of visual images. *Cogn. Psychol.* **5**, 138–150.

O'CONNOR, N. & HERMELIN, B. (1972). Seeing and hearing in space and time. *Percept. Psychophys.* **11**, 46–48.

PIAGET, J. & INHELDER, B. (1971). *Mental Imagery in the Child*. London: Routledge & Kegan Paul.

PIAGET, J. & INHELDER, B. (1973). *Memory and Intelligence*. London: Routledge & Kegan Paul.

ROCK, I. (1974). The perception of disoriented figures. *Sci. Amer.* **230**, 78–85.

SHEPHARD, R. N. & FENG, C. (1972). A chronometric study of paperfolding. *Cogn. Psychol.* **3**, 228–243.

SHEPHARD, R. N. & METZLER, J. (1971). Mental rotation of three-dimensional objects. *Science, N.Y.* **171**, 701–703.

SULLIVAN, E. V. & TURVEY, M. T. (1972). Short-term retention of tactile stimulation. *Q. J. exp. Psychol.* **24**, 253–261.

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SPATIAL PERCEPTION IN THE BLIND

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It is often assumed that the spatial senses (vision, hearing and the tactual senses) operate as distinct and independent modalities and, moreover, that vision is crucial to the development of spatial abilities. However, well controlled studies of blind persons with adequate experience show that they can function usefully in space. In other words, vision is not a necessary condition for spatial awareness. On the other hand, though the blind may be equal or even superior to the sighted when performing spatial tasks within the body space, they may be deficient, either developmentally or absolutely, in tasks which involve events at a distance from the body, principally in auditory localization. One possible explanation of the differences between blind and sighted (McKinney, 1964; Attneave & Benson, 1969; Warren, 1970) is that vision is the primary spatial reference, and inputs from other modalities are fitted to a visual map. Several criticisms of this theory are adduced and an alternative theory derived from Sherrington (1947), in which all sensory inputs map on to efferent patterns, is sketched.

Comparisons of perceptual abilities in blind* and sighted† people have often tried to estimate the effects of loss or absence of vision on spatial perception. There have been two distinct lines of theoretical preference. The first has treated the spatial senses (vision, hearing and tactual perception) as autonomous and distinct modes of perceiving the world and frequently the argument has been for a visual-dominance hypothesis. Von Senden (1960), for example, thought that all real knowledge of space, including the concept of space itself, is gained through vision alone, and despite its many conceptual and empirical difficulties (see Gregory & Wallace, 1963) his thesis has frequently been accepted (e.g. Hebb, 1949; Hartlage, 1969). The extreme theory that the blind person has no concept of space is difficult to take seriously on logical grounds. It has been argued since Kant that the concept of space is *a priori* to spatial judgements and not abstracted from *any* empirical evidence. It is certainly an empirical question whether one object is, say, to the right of the other, but there is no conceivable empirical evidence which could invalidate the claim that two objects are in *some* spatial relationship.

In its weaker form the von Senden theory is that knowledge of spatial relationships built upon auditory and tactual cues will be inferior to that based on vision and that such knowledge is useless in the interpretation of visual information. Von Senden was concerned to argue against traditional notions that 'touch teaches vision' and so he assumed like Rock (1966) that vision must teach touch. However, there is no need to conceptualize the relationship between the senses in dominance terms. A very different line of theorizing has been that the spatial senses operate not as distinctly differentiated modalities but as a unitary system (see, e.g., Freedman, 1968). Whereas the von Senden theory would predict that the blind person would have little or no awareness of space since vision is held to be crucial to the exercise of the concept of space in spatial judgements, the notion that the senses are interdependent in spatial perception would lead us to predict particular deficits in the blind. Blindness may mean lower ability in tasks requiring higher-order integration of information, though there may be modality specific 'threshold' tasks in which the blind through practice become superior to the sighted.

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* Unless otherwise stated, the word 'blind' refers to persons congenitally blind who have, at the most, perceptions of light. Psychologists have not always been careful to distinguish between the congenitally blind and the late-blind, though there may be important differences between the groups (e.g. Axelrod, 1959), nor between those without residual visual abilities and those who, though 'legally blind', may have some useful vision.

† Unless otherwise stated, in all the comparisons of blind and sighted persons reviewed here the sighted subjects were blindfolded or otherwise operating without visual information.

GENERAL MOBILITY

Day-to-day mobility depends upon a knowledge of spatial relations not immediately present to the senses, so that adequate functioning in space on the part of the blind person would indicate that vision is not crucial to the development of 'spatial awareness'.

Hartlage (1969) found that blind children (grade 2–12) made more errors than matched sighted controls in answering spatial questions, though the groups did not differ in their handling of non-space questions. He argued therefore that vision is a pre-requisite for the development of spatial abilities. However, his results may only reflect a relative inability on the part of blind children to exercise spatial concepts in the abstract rather than in actual spatial orientation. There is evidence that even in the sighted the verbal expression of spatial concepts develops later than the ability to manipulate spatial relations (e.g. Asso & Wyke, 1970).

Several mobility studies have in fact shown that the blind may have adequate spatial abilities. Leonard & Newman (1967) found that blind boys could follow a route in an unfamiliar area from memory of a tactual map and were also able to solve two detour problems en route. Clearly the blind matched ordinary criteria for spatial orientation abilities and the difficulties which they may sometimes have in spatial perception and orientation may be due to a lack of adequate experience rather than to a lack of vision. Support for the experiential hypothesis comes from Gomulicki's (1961) study of sighted and blind children in the age range 5–15 years using both a stylus and a life-size maze .Though performance in both mazes was initially inferior in the blind, they had surpassed the sighted level of performance by 15 years (see also Brown, 1968).

If the blind develop spatial orientation skills more slowly than the sighted, there is no evidence that they cannot develop such skills. Consequently von Senden's (1960) theory that vision is a necessary condition for the acquisition of spatial concepts and abilities has been adequately disconfirmed.

MODALITY PROCESSES

Hearing

Hearing is the only modality which would allow the blind to anticipate objects beyond the reach of their own bodies. In at least some blind persons self-produced echo cues form the basis of obstacle detection (Rice, 1967). In this way the blind person can locate the position of objects (Rice *et al.*, 1965) and discriminate between them in size (Rice & Feinstein, 1965) so that echo ranging could provide minimal cues for form perception. Rice (1967) has, in fact, reported that four late-blind subjects could in this way discriminate between common shapes.

Curtis & Winer (1969) argued that changes in intensity rather than frequency are *a priori* more likely to provide the blind person with spatial information since human beings work within a far lower frequency range than, say, bats or porpoises. They found that expert blind travellers made significantly fewer errors than either homebound or sighted subjects in detection of changes in intensity while there was little difference between the three groups in detection of an increment in frequency. Benedetti & Loeb (1972) have similarly shown that blind subjects had both a higher hit rate and lower false alarm rate in detection of a randomly presented louder signal compared to sighted subjects.

Accuracy of relative localization of sounds appears to be similar in both blind and sighted. Gomulicki (1961) found little difference between blind and sighted (aged from 5 to 15 years) either indoors or out of doors in judgements of whether alternate sounds came from the same or different positions (see also Fisher, 1964). However, when subjects are required to establish the absolute position of a sound source the blind may be considerably inferior to the sighted. Gomulicki (1961) found that out of doors (the less complex situation acoustically) the blind required the age range 5–15 years to reach the sighted level of accuracy. Indoors, blind subjects were still inferior to the sighted at age 12 and showed no improvement beyond the 12-year level. Experimental work with animals by Spiegelman & Bryden (1967) gives support to Gomulicki's findings. They found that though early-blinded rats discriminated better than late-blinded rats between tones differing in frequency, the late-blinded animals were markedly superior in auditory localization.

Touch and kinaesthesis: simple sensory discrimination

A common sense presupposition, supported by several studies, is that the blind may acquire through practice finer cutaneous and kinaesthetic discriminations since they depend upon cutaneous sensitivity for Braille reading, etc. and upon kinaesthetic localization for everyday tasks.

Axelrod (1959) found that early-blind subjects had significantly lower two-point cutaneous thresholds compared to sighted and late-blind groups on the right index finger which was most commonly involved in Braille reading. I have shown that blind children may be more accurate and less variable than sighted children, both in localization of a cutaneous point and in kinaesthetic localization measured by the ability to accurately duplicate a given movement (Jones, 1972*a*). Similarly, Hunter (1954) found lower thresholds on average for blind compared to sighted children in tactual–kinaesthetic detection of curvature. Hunter's findings were confirmed by Davidson (1972), who videotaped the subjects' exploratory movements and was able to show that the blind used more efficient strategies than the sighted. In general, the blind made significantly more use of all four fingers and tended to explore the front edge of the stimulus rather more often.

Along with evidence that immediate perception of simple tactual qualities may be superior in the blind there are some indications that the blind person's storage of cutaneous and kinaesthetic information may be more efficient. Shagan (1970) found that the ability of the blind to retain a standard movement in short-term memory ('kinaesthetic memory') was superior to that of the sighted. Hill & Bliss (1968) had only one early-blind subject so that obviously no firm conclusions are possible but they did find that this subject had a short-term memory capacity for cutaneous stimuli more than twice that of three sighted and one late-blinded subject.

Tactual processes: higher order discriminations

Both Worchel (1951) and Axelrod (1959) have shown that early-blind children may make more errors of tactual form perception than either late-blind or sighted children. Foulke & Warm (1967), on the other hand, found that blind adults made fewer errors than matched sighted adults in comparing forms made up of Braille-like raised dots (all the blind subjects were skilled Braille readers), suggesting in comparison to Worchel & Axelrod that the blind may acquire tactual skills more slowly than the sighted but ultimately that they develop more efficient skills. (Unfortunately, Foulke & Warm say only blind subjects were 'legally blind'. It may be that the group was made up of both early and late-blind subjects and that some subjects still had residual visual abilities.) Pick & Pick (1966) seemed to show no developmental decrease in error by blind subjects making same/different judgements of tactually perceived shapes. They did not, however, directly compare error rates in the blind and sighted at any age. Though the sighted did improve with age, Pick & Pick's graphs would indicate that the blind generally made fewer errors than the sighted at any age level. In other words, the performance of younger blind children was conceivably asymptotic.

Ewart & Carp (1963) who found that blind and sighted children made roughly the same number of errors in tactual matching of shapes also obtained a sizable correlation between IQ and matching for the blind, but not for the sighted (the blind had on average higher IQs than the sighted). Since IQ is by definition a developmental measure, Ewart & Carp's findings suggest that there may be no more than a developmental lag in tactual shape perception in the blind – the more intelligent blind subjects achieve the level of sighted performance at a comparable age and the others catch up later. Gomulicki (1961) also observed a correlation between intelligence and shape matching in the blind, with the more intelligent blind subjects at any age level performing as well as the sighted.

A central problem of interpretation here is that differences between the blind and sighted in matching shapes have been reported as differences in error (or correct response) rates. Such a procedure confounds factors of discriminative sensitivity with those of criterion placement or response bias (see, e.g., Green & Swets, 1966) – the blind could, for example, make more errors than the sighted in a 'same or different' matching task because they are more biased to give one response rather than another and not necessarily because they have lower discriminative sensitivity.

No study of perception in the blind has employed procedures which would allow independent estimates of sensitivity and bias. However, latency measures can, in principle, illustrate differences of criterion placement, since the shape matching task allows a wide choice of speed–accuracy trade-offs. Perhaps the simplest assumption is that latency is related to distance from criterion – the value of the evidence variable at which the subject will give a particular response (see, e.g., Norman & Wickelgren, 1969). Three studies have compared response times for shape matching between the blind and the sighted. Two studies with children used different procedures and obtained opposite findings for latency. Gomulicki (1961) used a many-alternative task, while Millar (1974) used a binary yes/no comparison. Gomulicki found that blind children took longer on average than the sighted and made more errors. Conceivably the blind set a higher decision criterion, which would be associated in a forced-choice procedure with an increase in false negative errors though not necessarily with lower sensitivity. Since blind adults in the only published comparison of recognition times in blind and sighted adults made fewer errors in less time than the sighted (Foulke & Warm, 1967), there may be changes with age in the nature of the decision strategies commonly used by the blind.

In contrast to Gomulicki, Millar (1974) found higher error rates in the blind associated with significantly faster responding. Such a speed–accuracy trade-off would be associated with an increase in false positive errors, though again not necessarily with lower sensitivity. In the absence of any study of the blind and sighted which separates errors of sensitivity and of bias or which associates latency with particular errors, the most reasonable conclusion may be that blind children tend to adopt a less than optimum response strategy and so make more errors, but there may be no differences of tactual sensitivity between the blind and the sighted in matching shapes.

Intermodal processes

Intersensory effects in blind persons have been investigated with reference to tactual–auditory transfer of training (Axelrod, 1959) and auditory–proprioceptive location (Fisher, 1964; Warren & Pick, 1970).

Axelrod (1959) found that early-blind subjects performed less well than the sighted on a learning-set task involving the transfer of a principle of solution from tactual to auditory means of presentation. Since he did not include within-modal controls (cf. Bryant, 1968) it is not possible to say whether the errors recorded by the blind in the cross-modal condition were due to some failure of intersensory integration or to an inability to process information in one or both of the modalities involved.

Fisher (1964) did find a significantly larger variable error for blind subjects in perceiving whether the second of two successive stimuli, either a felt position or a tone, was to the right or left of the first and, since there were no differences between the groups in discrimination of either two proprioceptive or two auditory locations, it may be argued that there is some breakdown of intersensory integration in the blind. However, this conclusion is not as tight as it appears. Fisher's data showed that the high mean variable error by the blind can be attributed to a hugely disproportionate error by two out of five of the blind subjects. The remaining three blind persons were, in fact, more consistent than all but two of the sighted.

Warren & Pick (1970) used a conflict paradigm in which the subject touched a small speaker while the auditory signal was laterally displaced by means of pseudophones. For the sighted there was a tendency for auditory bias of proprioception to increase and proprioceptive bias of audition to correspondingly decrease over an age range of second grade, sixth grade and adult. The blind, however, showed no change over age for either bias effect. Warren & Pick's (1970) interpretation of their results in terms of the interdependence of modality dominance effects with age will be discussed below.

DISCUSSION

In the remainder of the paper I wish to discuss two theories which could account for the differences in perceptual abilities, in so far as there are differences, between the blind and the sighted. The two theories I shall call the 'visual map' theory and the 'motor organization' theory and I shall argue that the first, though widely assumed, is inadequate and that the second provides a useful alternative.

The visual map theory – a rather more sophisticated version of the visual dominance notion – is that vision must serve to organize spatial perception if the blind are poorer in, or slower to develop, spatial abilities. McKinney (1964), for example, argued that accurate finger localization depends upon a visual schema of the fingers (see also Attneave & Benson, 1969). Warren (1970) suggested similarly that the sighted can fix auditory position on a primary visual map of space which would necessarily be lacking in the blind, hence explaining their comparatively poorer auditory localization (Gomulicki, 1961).

A completely different theory can be based upon Sherrington's important suggestion that, because there are specific anatomical linkages between the sensory areas of the cortex and the motor cortex, the motor cortex may therefore be a pathway between the senses (Sherrington, 1947). This account implies that space perception depends upon a fusion of visual, auditory, cutaneous and proprioceptive inputs, but vision is only one element in a mutually supportive system rather than the primary spatial reference. Since the demonstration of spatial abilities requires at bottom the organization of movements in space, patterns of movement may serve to integrate spatial information.

Objections to the visual map theory

Firstly, the theory rests its case too early. Deficits of spatial perception in the blind may result not from the absence of organizational support but more simply from the absence of one element in a normally integrated system. For example, McKinney (1964) found that 8-year-old sighted children, compared to 4-year-olds, made fewer errors of finger localization (the task required the child to reverse his palm before pointing to the correct finger), and that there was no comparable development for the blind. He argued that a visual schema of the hand must be required to integrate changes of felt position. However, we need only say that finger localization requires *both* visual and tactual schemas (see, e.g., Halnan & Wright, 1960, 1961). The task would necessarily be difficult for the blind since only one schema can be present.

If the visual map is to be the primary spatial reference, it may either be given at birth or develop fairly quickly and prior to the ability to make auditory spatial judgements. Now the visual map may be conceptually an *a priori* requirement for non-visual spatial judgements without the theory implying any temporal priority. Warren (1970), however, argues for both the conceptual and the temporal priority of the visual map. He found that children in the light could no more accurately point to the source of a sound than in the dark, whereas adults were more efficient in the light. Hence Warren argued that the coordination of visual and auditory space is a matter of lengthy experience. Although he was able to show that *pointing* to an auditory target is more accurate with eyes open in adults but not in children, the study in which he demonstrated a visual effect on auditory acuity *per se* did not involve a developmental comparison. In a signal recognition experiment I found that children in grades 2, 4 and 7 had higher sensitivity for the position (right or left) of a tone with eyes open compared to when they were blindfolded (Jones, 1975). This effect of eyes open on auditory spatial acuity in younger subjects may perhaps have been overridden by movement response inaccuracies in Warren's (1970) earlier study. Since there is evidence that newborn infants can detect auditory–visual discrepancies (Aronson & Rosenbloom, 1971) the ability to make spatial judgements involving both auditory and visual reference may simply be a human given.

Warren (1970; Warren & Pick, 1970) argues in favour of Renshaw's (1930) hypothesis that the nature of the interdependence of the sensory modalities changes with age in a regular manner. Renshaw (1930; Renshaw & Wherry, 1931) found that cutaneous localization was more accurate with eyes shut during localizing ('tactile method') prior to the 12th year, beyond which accuracy was greater with eyes open ('visual method'). He argued, therefore, that there is a period of tactile followed by later visual dominance in normal perceptual development. However, no statistical analysis was made of the data and only a small number of subjects was used. In a replication I found that the 'visual' localization was consistently more accurate in children between 5 and 12 years and that there was no significant developmental increase in 'visual' accuracy. The 'tactile' method showed some improvement with age but was still not equal to the 'visual' method by age 12 (Jones, 1972*a*). Consequently, there is evidence against the notion, upon which Warren (1970) in some measure relies, that vision becomes a more important modality with age and in part against any theory that one modality may be dominant at a particular development stage.

More direct evidence against the theory that space is organized in visual terms comes from the work of Hermelin & O'Connor (1971), who found that blind children and blindfolded children and adults tend to pair an associate word with the finger touched rather than original spatial location when learning was tested with the hand reversed. With vision, however, both children and adults responded primarily in terms of visual location. Therefore spatial location cannot be organized in purely visual terms, though the work of Hermelin & O'Connor is not sufficient to show that space is independently structured by each modality.

The motor organization theory

Broadly, there are 'general' and 'specific' versions of the motor theory. The general theory is that all spatial distinctions are ultimately founded upon voluntary movements. Such a theory is basic to (say) Piaget's account of cognitive development (e.g. Piaget, 1969). Perception is construed as a family of skills which has 'developed in relation to the motor skills by which we deal with objects around us . . . A motor skill has no sharp boundaries; rather it is a capacity for dealing with a relatively indefinite range of objects in a relatively indefinite range of ways' (Taylor, 1971, p. 399). If the range of spatial distinctions which the child can make depends upon voluntary action, then lack of adequate voluntary movement experience would have perceptual effects. The work of Held and his colleagues (e.g. Held & Hein, 1963) can therefore be taken as support for the general theory, since they have tended to show that animals deprived of the opportunity to associate their voluntary movements with the visual environments suffer disorders of depth perception and of visually guided movement control (see also Riesen & Aarons, 1959).

The specific theory, acceptance of which is not dependent upon acceptance of the general version, is that particular movements may be involved in particular spatial judgements. On the specific theory, perceptual distinctions are not necessarily dependent *a priori* upon distinctions developed through movement (though distinctions *may* be developed in this way), but perceptual distinctions are made in part directly *through* movement. Hebb's (1949) suggestion, for example, was not that the activity of the motor cortex is essential to perception but that motor activity would serve to refine particular discriminations. I have provided some corroboration by showing that visual recognition of shapes is facilitated by voluntary (though not by passive) movement exploration of the shapes during observation (Jones, 1972*b*).

In practice, the general and specific theories as they apply to the blind may not be all that distinct. Since vision clearly does provide a rich source of information about things and events at a distance from the child's own body, the blind baby will have that much less reason to explore his environment, and, specifically, because the baby cannot reach for objects unless he knows they are near him, the blind may lack experience compared to the sighted of the particular patterns of movement necessary for tactual exploration. Further, White *et al.* (1964) have shown that the development of reaching movements may depend upon the baby's prior observation of his own moving limbs. It seems likely, therefore, that the blind baby would be retarded, both in the general motor control necessary to reach for objects and through such movements implicitly make distinctions like 'reach forward', 'reach to the right', etc., and in the development of the specific patterns of exploratory movements once objects are grasped. There is evidence from a study by Adelson & Fraiberg (1974) that congenitally blind children are much slower, particularly within the first year of life, in the development of independent self-initiated mobility. For the baby such immobility 'lessens his ability to explore independently, to discover by himself the objective rules that govern things and events in the external world' (Adelson & Fraiberg, 1974, p. 126).

Such retardation of development could conceivably persist to school age and beyond. However, I have tried to show that the evidence from comparisons of shape matching in blind and sighted children is equivocal and can be interpreted as differences in criterion placement rather than sensitivity. The major deficiency for the blind, both developmentally and perhaps absolutely, seems to be in the ability to localize sounds in space. Since auditory localization is of considerable importance to independent mobility in the blind, an adequate account of auditory localization may have some practical value.

Jones & Kabanoff (1975) have proposed a theory of auditory localization which depends upon the involvement of a *specific* movement pattern – target directed eye movements – in judging the position of a tone. We argued, following Kinchla & Allan (1969), that eye movements are a general means of updating and stabilizing position memory, and, more particularly, that eye movements may be involved in auditory localization. The work of Fishman & Michael (1973) could indicate the neural basis here. They have shown that there are cells in the visual cortex (of the cat at least) which are responsive to both visual and auditory stimuli, and which are probably linked to oculomotor nuclei.

If eye movements toward an auditory target can stabilize position memory, then 'noise' in the eye movement control system should interfere with auditory localization. It is known that auditory localization is poorer in the dark than in the light in both adults (Warren, 1970) and children (Jones, 1975) and there is considerable evidence that eye control in the dark is subject to involuntary drift (Becker & Klein, 1973; Matin *et al.*, 1966; Merton, 1961). The eyes may drift as much as 5°–10° before the subject is consciously aware of the need for correction (Becker & Klein, 1973). Voluntary eye movements toward a target would therefore be less accurate because superimposed upon a pattern of 'noisy' drift.

Platt & Warren (1972) and Jones & Kabanoff (1975) have shown that auditory localization is less accurate in the absence of eye movements. The eye movement could therefore serve to confirm the initially auditory judgement. We also showed that cueing eye movement away from the source of a sound led to a substantial decline in localization accuracy compared to making no eye movement. In other words, subjects may be more likely to base their final judgement of auditory position upon the direction of their eye movement rather than upon the primary acoustic cues. Platt & Warren (1972) argued that eye movements combine with visual afference to fix auditory position. However, Jones & Kabanoff (1975) found that in some circumstances visual position can only be accurately fixed from auditory position, and also that eye movements can be cued to an auditory target in total darkness. In other words, visual afference is not a necessary condition for accurate auditory localization, but a visual background ordinarily does serve to correct involuntary eye drift.

The blind person attempting to localize a sound from a fixed position may be compared to the sighted person in the dark. Control of eye movements is likely to be poor, since the blind person has little information with which to correct involuntary drift (in a number of cases the eyes may even have been enucleated), and a convenient means of confirming auditory position is unavailable. Accurate control of eye movement has ordinarily been thought to serve little purpose in the blind, yet it may be possible to facilitate auditory localization in many blind persons by using a verbal cueing technique for eye movement (Jones & Kabanoff, 1975).

Warren (1970), in suggesting the visual map theory, supposed that there would be a different basis for the organization of spatial abilities in the blind. The motor theory can avoid this necessity. Where the nature of blindness precludes the easy acquisition of a pattern of movements directly involved in spatial judgements by the normally sighted, we can expect deficits of spatial perception in the blind. It may also be that the development of spatial accuracy proceeds more slowly in the blind in so far as the handicap interferes directly or indirectly with the person's independent mobility.

The present review has also shown that the blind may develop, rather more quickly than the sighted, skills which broadly speaking involve more accurate perception of events within the child's own body space, e.g. cutaneous localization (see Axelrod, 1959; Jones, 1972) or the perception of movements of his body within a predetermined space (see Jones, 1972; Shagan, 1970), whereas the perception of events in the spatial world is likely to develop more slowly, e.g. auditory localization (Gomulicki, 1961). Tactual form perception is of especial interest here, since this ability overlaps the body-space/world distinction. Unfortunately, there is as yet no clear-cut evidence on the development of tactual perception in the blind, and well-controlled comparisons of the blind and the sighted would be useful. On the other hand, tactual perception may be more accurate in blind adults (Foulke & Warm, 1967), implying that the link between body-space and the world is voluntary movement. By acquiring more efficient patterns of voluntary movement (Davidson, 1972) the blind person acquires more accurate perceptions of things in the world.

Of course, some perceptual abilities in the blind (or the sighted) may develop in relative isolation from each other. The blind may develop and refine abilities which the sighted would have little reason to practice, and the sort of experience necessary to acquire cutaneous sensitivity may not be like that necessary to appreciate proprioceptive input. That is, there may be within-modality effects which are relatively independent of whatever relationships exist between the senses. However, the question here is not whether some perceptual skills can be practised independently of others, but in reference to the visual dominance theories, whether such skills *must* remain isolated in the absence of vision. There is no evidence from the present review to support any version of the visual dominance idea and consequently no reason to think that perceptual skills in the blind of necessity remain diffuse. Of course, within-modal skills may only be integrated in the sense that the same spatial judgement can be based upon different sensory information, i.e. the same voluntary action could result from any number of sensory patterns. It is particularly interesting to note that perceptual skills which are not on the face of it spatial skills may develop more efficiently in the blind compared to the sighted in relation to independent mobility. The blind may have, for example, a more accurate loudness discrimination than the sighted (Benedetti & Loeb, 1972; Curtis & Winer, 1969), particularly if they are experienced travellers, though they may not develop any more precise discrimination of pitch (Curtis & Winer, 1969).

In summary, there is no evidence for the proposition that the blind as a consequence of loss of vision suffer deficits of spatial perception in the remaining modalities. At the moment, what deficits there are in the blind can be traced to inadequate experience of general mobility (e.g. Leonard & Newman, 1967) and in some cases to the relative inability to develop particular patterns of movement (cf. Jones & Kabanoff, 1975).

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REFERENCES

ADELSON, E. & FRAIBERG, S. (1974). Gross motor development in infants blind from birth. *Child Dev.* **45**, 114–126.

ARONSON, E. & ROSENBLOOM, S. (1971). Space perception in early infancy: perception within a common auditory-visual space. *Science, N.Y.* **172**, 1161–1163.

ASSO, D. & WYKE, M. (1970). Visual discrimination and verbal comprehension of spatial relations by young children. *Br. J. Psychol.* **61**, 99–107.

ATTNEAVE, F. & BENSON, B. (1969). Spatial coding of tactual stimulation. *J. exp. Psychol.* **81**, 216–222.

AXELROD, S. (1959). *Effects of Early Blindness.* New York: American Foundation for the Blind.

BECKER, W. & KLEIN, H. M. (1973). Accuracy of saccadic eye movements and maintenance of eccentric eye positions in the dark. *Vision Res.* **13**, 1021–1034.

BENEDETTI, L. H. & LOEB, M. (1972). A comparison of auditory monitoring performance in blind subjects with that of sighted subjects in light and dark. *Percept. Psychophys.* **11**, 10–16.

BROWN, G. D. (1968). Evaluation of an orientation mobility, and living skills workshop for blind children. *Except. Child* **35**, 239–240.

BRYANT, P. E. (1968). Comments on the design of developmental studies of cross-modal matching and cross-modal transfer. *Cortex* **4**, 127–137.

CURTIS, J. F. & WINER, D. M. (1969). The auditory abilities of the blind as compared with the sighted. *Aud. Res.* **9**, 57–59.

DAVIDSON, P. W. (1972). Haptic judgments of curvature by blind and sighted humans. *J. exp. Psychol.* **93**, 43–55.

EWART, A. G. & CARP, F. M. (1963). Recognition of tactual form by sighted and blind subjects. *Am. J. Psychol.* **76**, 488–491.

FISHER, G. H. (1964). Spatial localization by the blind. *Am. J. Psychol.* **77**, 2–13.

FISHMAN, M. C. & MICHAEL, C. R. (1973). Integration of auditory information in the cat's visual cortex. *Vision Res.* **13**, 1415–1419.

FOULKE, E. & WARM, J. S. (1967). Effects of complexity and redundancy on the tactual recognition of metric figures. *Percept. mot. Skills* **25**, 177–187.

FREEDMAN, S. J. (1968). *The Neuropsychology of Spatially Oriented Behaviour.* Homewood, Ill.: Dorsey, Press.

GOMULICKI, B. R. (1961). *The Development of Perception and Learning in Blind Children.* Private publication from the Psychological Laboratory, Cambridge University.

GREEN, D. M. & SWETS, J. A. (1966). *Signal Detection Theory and Psychophysics.* New York: Wiley.

GREGORY, R. & WALLACE, J. (1963). Recovery from early blindness. *Exp. Psychol. Monogr.* Whole no. 2.

HALNAN, C. R. E. & WRIGHT, G. H. (1960). Tactile localization. *Brain* **83**, 677–700.

HALNAN, C. R. E. & WRIGHT, G. H. (1961). Fingers and toes in the body-image. *Acta neurol. scand.* **37**, 50–61.

HARTLAGE, L. C. (1969). Verbal tests of spatial conceptualization. *J. exp. Psychol.* **80**, 180–182.

HEBB, D. O. (1949). *The Organization of Behaviour.* New York: Wiley.

HELD, R. & HEIN, A. (1963). Movement-produced stimulation in the development of visually guided behaviour. *J. comp. physiol. Psychol.* **56**, 607–613.

HERMELIN, B. & O'CONNOR, N. (1971). Spatial coding in normal, autistic and blind children. *Percept. mot. Skills* **33**, 127–132.

HILL, J. W. & BLISS, J. C. (1968). Modelling a tactile sensory register. *Percept. Psychophys.* **4**, 91–101.

HUNTER, I. M. L. (1954). Tactile–kinaesthetic perception of straightness in blind and sighted humans. *Q. J. exp. Psychol.* **6**, 149–154.

JONES, B. (1972*a*). The development of cutaneous and kinaesthetic localization in blind and sighted children. *Dev. Psychol.* **6**, 349–352.

JONES, B. (1972*b*). Facilitation of visual perception through voluntary movement in elementary school children. *J. exp. child Psychol.* **14**, 408–415.

JONES, B. (1975). Visual facilitation of auditory localization in school children: a signal detection analysis. *Percept. Psychophys.* **17**, 217–220.

JONES, B. & KABANOFF, B. (1975). Eye movements in auditory space perception. *Percept. Psychophys.* **17**, 241–245.

KINCHLA, R. A. & ALLAN, L. G. (1968). A theory of visual movement perception. *Psychol. Rev.* **76**, 537–558.

LEONARD, J. A. & NEWMAN, R. C. (1967). Spatial orientation in the blind. *Nature, Lond.* **215**, 1413–1414.

MCKINNEY, J. P. (1964). Hand schema in children. *Psychon. Sci.* **1**, 99–100.

MATIN, L., PEARCE, D., MATIN, E. & KIBLER, G. (1966). Visual perception of direction in the dark: roles of local sign, eye movements and ocular proprioception. *Vision Res.* **6**, 453–469.

MERTON, P. A. (1961). The accuracy of directing the eyes and the hand in the dark. *J. Physiol., Lond.* **156**, 555–557.

MILLAR, S. (1974). Tactile short-term memory by blind and sighted children. *Br. J. Psychol.* **65**, 253–263.

NORMAN, D. A. & WICKELGREN, W. A. (1969). Strength theory of decision rules and latency in retrieval from short-term memory. *J. math. Psychol.* **6**, 192–208.

PIAGET, J. (1969). *The Child's Conception of the World.* London: Routledge & Kegan Paul.

PICK, A. D. & PICK, H. L. (1966). A developmental study of tactual discrimination in blind and sighted children and adults. *Psychon. Sci.* **6**, 367–368.

PLATT, B. B. & WARREN, D. H. (1972). Auditory localization: the importance of eye movements and a textured visual environment. *Percept. Psychophys.* **12**, 245–248.

RENSHAW, S. (1930). The errors of cutaneous localization and the effects of practice on the localizing movement in children and adults. *J. genet. Psychol.* **38**, 223–238.

RENSHAW, S. & WHERRY, F. J. (1931). Studies on cutaneous localization. III. The age of onset of ocular dominance. *J. genet. Psychol.* **39**, 493–496.

RICE, C. E. (1967). Human echo perception. *Science, N.Y.* **155**, 656–664.

RICE, C. E. & FEINSTEIN, S. H. (1965). Sonar system of the blind: size discrimination. *Science, N.Y.* **148**, 1107–1108.

RICE, C. E., FEINSTEIN, S. H. & SCHUSTERMAN, R. J. (1965). Echo-detection ability of the blind: size and distance factors. *J. exp. Psychol.* **70**, 246–251.

RIESEN, A. H. & AARONS, L. (1952). Visual movement and intensity discrimination in cats after early deprivation of pattern vision. *J. comp. physiol. Psychol.* **59**, 142–149.

ROCK, I. (1966). *The Nature of Perceptual Adaptation.* New York: Basic Books.

SENDEN, M. VON (1960). *Space and Sight.* London: Methuen.

SHAGAN, J. (1970). Kinaesthetic memory, comparing blind and sighted subjects. (Unpublished dissertation, George Washington University.)

SHERRINGTON, C. S. (1947). *The Integrative Action of the Nervous System.* Cambridge: Cambridge University Press.

SPIEGELMAN, M. N. & BRYDEN, M. P. (1967). Effects of early and late blindness on auditory spatial learning in the rat. *Neuropsychol.* **5**, 267–274.

TAYLOR, C. (1971). What is involved in a genetic psychology. In T. Mischel (ed.), *Cognitive Development and Epistemology.* New York: Academic Press.

WARREN, D. H. (1970). Intermodality interactions in spatial localization. *Cogn. Psychol.* **1**, 114–133.

WARREN, D. H. & PICK, H. L. (1970). Intermodality relations in localization in blind and sighted people. *Percept. Psychophys.* **8**, 430–433.

WHITE, B. L., CASTLE, P. W. & HELD, R. M. (1964). Observations on the development of visually-directed reaching. *Child Dev.* **35**, 349–364.

WORCHEL, P. (1951). Space perception and orientation in the blind. *Psychol. Monogr.* **67**, no. 3. (Whole no. 332.)

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DEVELOPMENTAL CHANGES IN CONSENSUS IN CONSTRUING WITHIN A SPECIFIED DOMAIN

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Developmental changes in the amount of social consensus in grid ratings are investigated in six samples of school children spanning the age range 6–17. The grids studied use supplied constructs and elements elicited to represent various categories of stories (e.g. favourite, hard). On these grids there is increasing consensus in construing across the age span studied. This consensus is substantially higher for patterns of inter-construct relationships (the *structure* of the system) than for ratings of specific elements on specific constructs (the *implications* of the system). The results also suggest that systematic relationships can be expected between group and individual patterns of response, when the structure is considered in terms of specific 'interpreted' dimensions of variation. Correspondence between group and individual patterns at this more global level does not show any clear developmental pattern. Instead, the lesser consensus of the younger subjects seems to reflect a more idiosyncratic patterning of the major dimensions of construing, within which the dimensions themselves are preserved.

BACKGROUND

It is a basic principle of personal construct theory (Kelly, 1955) that psychological processes should be considered in the individual rather than referred to the 'average' performance of a group. This does not rule out concern with similarities in the way various members of a given social group construe experience, however. Kelly deals with this in his commonality corollary, which states that 'to the extent that one person employs a construction of experience which is similar to that employed by another, his psychological processes are similar to those of the other person' (p. 90). The presence of such consensus in patterns of construing is the basis of a shared or common cultural experience, providing a general framework within which each individual develops his own unique construction of the world.

The present study uses repertory grid techniques to study the development of such a cultural framework in a specified domain: the 'spectator role' in language use (Britton, 1970, 1971). This is a role in which the person is 'looking on' rather than 'participating in' (Harding, 1937) the activities and experiences with which he is concerned, and embraces the traditional forms of both 'high' and 'low' culture. Attention was restricted here to 'stories' as a central example of the spectator role, though these were broadly defined to include narratives of any length, including fairy tales, short stories, and novels of all kinds.

In the grids, stories were taken as the *elements* from the world at large, to be rated on a set of *constructs* used to understand and order the elements. The analysis considered consensus in two areas: (1) ratings of specific elements on specific constructs, and (2) relationships between constructs (the 'structure' of the system).

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METHOD

Instruments

Two grids were designed to study construing in the spectator role: an orally administered grid for younger children, and a written grid administered as a questionnaire for older ones. The written grid included 19 supplied construct poles derived from previous investigations (especially Carver, 1967) and preliminary studies, and elicited a 20th construct from the pupils.

Elements to be rated were elicited by providing subjects with a list of eight story categories (e.g. favourite, moving, hard) to illustrate from their own experience; one additional title (*Cinderella*) was rated by all subjects. The story categories sought to provide a representative, though clearly not a random, selection from the domain of interest.

For the oral grids, the list of constructs was restricted to 10 appropriate for younger subjects; seven constructs were common to both the oral and the written grids. Eight story titles were rated by each child, but in this case the titles were elicited from the class teachers in response to a similar list of categories (e.g. 'A story the class in general enjoys').

Five-point grading scales were used on both grids. Copies of the grids are available in Applebee (1973).

Samples

Initial samples were drawn from an infant, a junior and a comprehensive school in a single north London drawing area. Twenty-two subjects at 6 and 22 at 9 completed the oral grid. Thirty at 9 and 30 at 13 completed the written measure. At 9, children completing the oral and written versions of the grid were drawn at random from the same classes; those completing the written measure are treated as part of a 'comprehensive school' sample in the discussion below.

Separate samples of 20 pupils at 13 and 20 at 17 were drawn from a suburban London boys' selective school and its associated girls' school. These samples were dominated by children from social classes I and II (professional and intermediate) using the General Registry Office (1966) classification of parental occupations; those from the north London schools were dominated by social class III (skilled workers), with lesser proportions of classes II and IV.

Vocabulary ability, as measured by the Mill Hill Vocabulary Scale (Raven, 1965), averaged 104·1 for the 6-year-olds and 107·1 for the 9-year-olds. Verbal reasoning ratings based on testing at entry from primary school averaged 110·2 for the comprehensive school children. Average ages fell about 8 months into each age group sampled, except for the 6-year-old sample which had an average age of only 5;11.

All samples had equal numbers of boys and girls at each age.

Measures

Consensus. Consensus in ratings of particular elements was assessed by calculating intra-class correlation coefficients for each age group (Snedecor & Cochran, 1967). These correlations range from 0·0 when subjects show only random agreement, to 1·0 when they agree completely in their ratings of particular stories on all of the constructs.

As a measure of the structure of the construct system, within-grid correlations between constructs were calculated for each subject and expressed in angular distances; intra-class correlations were then calculated as summary measures of the amount of consensus about the inter-construct relationships. These were computed using Slater's (1972*a*) COIN programme.

Distributions for intra-class correlation coefficients are complex though known (Kendall & Stuart, 1966, p. 315); in the present study McNemar's (1969, p. 322) suggested use of the significance of the between groups mean square relative to the residual mean square was followed, with degrees of freedom based on the conservative test.

Alternative measures of consensus about structure were derived from principal components analyses of the sums of squares and cross products for each age group, calculated using a 'stringing out' procedure (Maguire, 1973; Miron & Osgood, 1966) in which each vector of ratings of a given element is treated as a separate observation.

If there is no consensus within a sample about inter-construct relationships, then those relationships should tend towards zero in the group matrix; conversely, to the extent that

Table 1. *Consensus in ratings of selected story-types on all constructs*

	Oral grids		Written grids			
			Comprehensive school		Selective schools	
	Age 6	Age 9	Age 9	Age 13	Age 13	Age 17
Intra-class correlation	0·004	0·087	0·140*	0·322**	0·388**	0·318**
Average titles/story-type	2·7	3·0	22·4	23·5	18·8	17·9
No. of cases	22	22	30	30	20	20

* $P < 0·05$; ** $P < 0·01$.

the group matrix shows definite non-zero patterns of organization we can infer some group consensus to be present. As measures of organization in the group matrices, three measures were tabled: (i) the proportion of correlations reaching the 0·05 significance level, two-tailed; (ii) the proportion of variation accounted for by the first principal component; and (iii) the centrality of the first component, calculated as the proportion of constructs whose loadings on it are equal to 20 per cent or more of the average per construct variation.

Interpretation of components. A principal components analysis was also carried out on each of the individual grids, using Slater's (1965, 1972*b*) INGRID programme. As with the group analyses, scores were not normalized, in order to preserve (presumed real) differences in inter-construct variability.

Since a substantial proportion of the constructs dropped out of the oral grids because of lack of variation, no attempt was made to interpret the components emerging from these analyses. Instead, each component was considered to be 'oriented' towards the construct which loaded most highly on it. The orientation of each of the first three components of each of the individual grids was then tallied; in the case of tied loadings, the tied constructs were all tallied.

For the larger written grids it was possible to carry out a more thorough analysis. Each of the first three components was separately interpreted, by specifying a superordinate construct which subsumed the constructs with large loadings. These classifications were made before the analyses of group structure were undertaken; later the group analyses were similarly interpreted and the two sets of analyses compared. Labelling the components in this way makes it simpler to compare results from different grids, but is of course no more than a convenient shorthand for a complicated statistical pattern.

RESULTS

Consensus in ratings

Table 1 summarizes the results from the analysis of agreement in rating particular stories on specific constructs. Though the intra-class correlations remain quite low at all ages, never rising above 0·39, there is a clear rise with age from the 6- to the 13-year-old samples.

At nine, there is more agreement in ratings on the written grids (where the examples were selected by each subject to represent the general categories) than in ratings on the oral grids (where titles were selected by each teacher for all members of her class). Seven constructs are common to both grids: for each of the seven, the intra-class correlation is higher for the written grids than for the oral ones. (This pattern of differences is significantly different from chance, $P < 0·02$, two-tailed, using the binomial test.) The average intra-class correlation (using the z transformation) on these seven constructs is 0·069 for the oral grids, and 0·247 for the written ones.

Table 2. *Consensus in inter-construct relationships*

			Written grids			
	Oral grids		Comprehensive school		Selective schools	
	Age 6	Age 9	Age 9	Age 13	Age 13	Age 17
Intra-class correlation	0·014	0·184*	0·193**	0·458**	0·512**	0·552**
No. of cases	22	22	30	30	20	20

* $P < 0·05$; ** $P < 0·01$.

Consensus in structure

Although ratings of specific grid elements on specific constructs may differ from person to person, it is still possible that the interrelationships among the constructs will be quite similar: John and Barbara may agree that an 'exciting' story is one they 'like', even if they disagree completely about which particular story they will construe as 'exciting'. Because of this, consensus in inter-construct relationships as well as in ratings was examined. The results, reported in Table 2, indicate somewhat more consensus than was apparent in the analysis of ratings. Again beginning at essentially zero at age 6, the intra-class correlation rises to a high of 0·55 in the 17-year-old sample.

There is some difficulty with this measure for the oral grids, however, because of the tendency of the younger children not to discriminate amongst the stories on some of the constructs. This in turn produces constructs with no variation, and no estimate of the correlation between the construct and others in the grid. In the calculations of intra-class correlations, these were taken as the zero-order correlations they statistically represent, but this zero-order correlation is sometimes at the furthest possible extreme from the correlation that emerges when there *is* variation in the ratings. The constructs 'very good' and 'one I like', for example, are highly correlated in all of the grids where both have variation, but show no variation and have a zero correlation in a substantial proportion of the oral grids. The magnitude of this effect is evident in the 6-year-old sample: here the average correlation between the two constructs is 0·29 if all grids are considered, but 0·95 if only those grids in which there is variation on both constructs are included in the calculations.

The tendency for the variation on some constructs to be restricted can cause difficulties even when some variability is preserved; with most of the elements clustered at one point on a rating scale, correlations between constructs are sometimes determined by one or two deviant ratings. These may in turn be shaped more by the particular characteristics of the elements being rated than by the general relationships between the constructs in question. (Similar difficulties led Bannister to constrain subjects to divide elements equally between poles, and in other studies to rank-order them; see Bannister & Fransella, 1971, p. 71.)

These difficulties lead us to look as well at estimates of organization in the pooled matrix for each age group, which will be less subject to such distortions. Table 3 summarizes the results from the three measures of organization. As with the

Table 3. *Measures of organization in the group matrix*

Measures	Oral grids		Written grids			
			Comprehensive school		Selective schools	
	Age 6	Age 9	Age 9	Age 13	Age 13	Age 17
Var. expl. 1st comp. (%)	21·0	25·6	19·9	34·4	32·5	34·7
Significant correlations (%)	38·8	48·9	48·4	82·6	76·8	82·6
Centrality 1st comp. (%)	40·0	40·0	45·0	60·0	70·0	85·0
No. of elements rated, pooling subjects	176	176	270	270	180	180

Table 4. *Orientation of first three components: oral grids*

Construct with highest loading	Percentage of grids		Construct with highest loading	Percentage of grids	
	Age 6	Age 9		Age 6	Age 9
1	18·2	27·3	6	18·2	18·2
2	40·9	59·1	7	36·4	36·4
3	40·9	9·1	8	31·8	9·1
4	45·5	50·0	9	36·4	63·6
5	40·9	0·0	10	0·0	18·2

n = 22 grids at each age; components with tied loadings are classified under each construct.

intra-class correlations, these analyses show a rise across the age range 6–17, with the largest jump occurring between the 9- and 13-year-old samples. Though they must be treated with caution because they are not based on independent observations, the high proportion of statistically significant correlations even in the 6-year-old sample suggests there may in fact be some consensus about structure even at this level.

Individual and group components

The group structure for the 6-year-olds completing the oral grids is relatively diffuse, with little tendency for constructs to coalesce into clearly separate dimensions. At 9, the group structure has become more sharply focused, with clear dimensions corresponding to the 'simplicity' and 'evaluation' of the stories; a third dimension was defined by a single construct, 'teaches a lesson', which separated school reading books from others rated. (These results are presented in full in Applebee, 1973.)

The orientations of components in the individual oral grids at 6 and 9 are summarized in Table 4. As with the analyses of the group matrix, there is a sharper pattern in the older sample; constructs 9, 4 and 2 dominate. These represent an evaluation, a judgement of simplicity, and 'teaches a lesson', respectively. At 6 there is a more even distribution among the 10 constructs. Rather than completely unique individual patterns of relationships, however, comparisons of group and individual structures suggest that the individual construct systems as measured by these grids involve primarily a rotation of the principal axes towards one or another construct in the group system, with the whole structure tightening up around it in a 'halo' effect and, especially at 6, many of the other constructs dropping out of the system altogether. There is no evidence that the individual

Table 5. *Interpretation of the first three components: written grids*

	Percentage of grids			
	Comprehensive school		Selective schools	
Interpreted component	Age 9	Age 13	Age 13	Age 17
A. Evaluation	93·3	100·0	95·0	90·0
B. Simplicity	66·7	73·3	55·0	65·0
C. Realism	26·7	36·7	20·0	60·0
D. Seriousness	16·7	20·0	70·0	25·0
Unclassified	96·7	70·0	60·0	60·0
Grids with A, B and C	23·3	33·3	10·0	35·0
Grids with 2 of A, B and C	70·0	56·7	70·0	60·0
Grids with 1 of A, B and C	6·7	10·0	20·0	5·0
No. of cases	30	30	20	20

patterns involve major realignments, such as a bringing together of constructs from opposite ends of the group system.

For the written grids, the group analyses showed three major components which occurred across the age groups; these were interpreted as 'evaluation', 'simplicity' and 'realism'. In the analyses of the individual grids, these three components similarly appeared frequently, augmented by a fourth component, 'seriousness', which was especially prominent in the 13-year-old selective school samples. This fourth component reflected a temporary preoccupation with 'serious' adult literature, in which these subjects had only recently become interested.

Table 5 summarizes the individual components. The frequency with which each occurs is related quite directly to its importance in the analyses of the group structure, suggesting a considerable overlap between the two approaches. If the three largest components are considered simultaneously, nearly 90 per cent of the individual grids show two of the three major group dimensions; on the other hand, less than one-third of the individual grids reflect the group patterns in full. Interestingly, though the intra-class correlations are higher for the older samples, there is no tendency for the individual grids in the older samples to correspond more closely to the group pattern in terms of the proportion of interpreted components which appear. Apparently, the variability of the younger children occurs within the major dimensions of variation, rather than altering the dimensions themselves.

DISCUSSION

The results as a whole show a clear rise with age in the amount of social consensus in construing, within the domain sampled here. This convergence is much more marked for patterns of relationships among constructs than for the ratings of specific elements on specific constructs: there is more agreement about the *structure* of the construct system than about its *implications*, at least when structure is considered at this relatively superordinate level. If we consider relative contributions to the total variation, the differences between readers far outweigh the similarities in their responses to titles selected to represent such categories as 'favourite', 'disliked', 'moving' or 'difficult' stories; in turn, there is some evidence that agreement about these story *categories* is greater than agreement about specific titles. Such diversity in response emphasizes the difficulty inherent in any attempt to specify the characteristics of books which will 'appeal' to readers at various selected ages, in spite of the old and extensive research literature which has been devoted to that task (cf. Mott, 1970).

The results also suggest that systematic relationships can be expected between group and individual patterns of response, when the structure is considered in terms of specific, 'interpreted' dimensions of variation. When we are concerned with differences between groups, inferences from the group matrices studied here are similar to those from separate analyses of the structure for each individual. The degree of correspondence between group and individual patterns at this more global level does not show any clear developmental pattern. With the possible exception of the 6-year-old sample, subjects at each of the ages studied here seem to share with other subjects at the same age a common, generalized framework for construing the stories they encounter. For the younger subjects, however, the precise definitions of the shared dimensions show more idiosyncratic patterning: while nearly all may have an 'evaluative' dimension, the specific constructs which are central to it will vary from individual to individual. As age increases, there seems to be a gradual focusing and refinement of each dimension for each subject, bringing the dimension into closer alignment with that of other subjects in the group.

The issue on which the present data do not provide evidence concerns whether the diversity at the younger ages is the result of *stable individual* differences, or *lack of stable structure* in individual patterns of response. The latter is probably an especially important source of variation for the 6-year-olds, for whom we would expect to find syncretistic patterns of thinking and some intransitivity in patterns of preference and response (Flavell, 1963; Bradbury & Nelson, 1974). Some of the observed variation is probably random, but some of it is likely to be systematic, reflecting the personal nature of the process of construing and leaving wide scope for the investigation of individual differences.

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REFERENCES

APPLEBEE, A. N. (1973). The spectator role. (Unpublished Ph.D. thesis, University of London.)

BANNISTER, D. & FRANSELLA, F. (1971). *Inquiring Man: the Theory of Personal Constructs.* Harmondsworth: Penguin Books.

BRADBURY, H. & NELSON, T. M. (1974). Transitivity and the patterns of children's preferences. *Dev. Psychol.* **10**, 55–64.

BRITTON, J. (1970). *Language and Learning.* London: Allen Lane The Penguin Press.

BRITTON, J. (1971). What's the use? A schematic account of language functions. *Educ. Rev.* **23**, 205–219.

CARVER, M. V. (1967). The critical evaluation of films by repertory grid. (Unpublished Ph.D. thesis, University of London.)

FLAVELL, J. H. (1963). *The Developmental Psychology of Jean Piaget.* Princeton, N.J.: Van Nostrand.

GENERAL REGISTRY OFFICE (1966). *Classification of Occupations*. London: H.M.S.O.

HARDING, D. W. (1937). The role of the onlooker. *Scrutiny* **6**, 247–258.

KELLY, G. A. (1955). *The Psychology of Personal Constructs*, vols. 1–2. New York: Norton.

KENDALL, M. & STUART, A. (1966). *The Advanced Theory of Statistics*. London: Griffin.

MCNEMAR, Q. (1969). *Psychological Statistics*, 4th ed. London: Wiley.

MAGUIRE, T. O. (1973). Semantic differential methodology for the structuring of attitudes. *Am. educ. Res. J.* **10**, 295–306.

MIRON, M. S. & OSGOOD, C. E. (1966). Language behaviour: the multivariate structure of qualification. In R. B. Cattell (ed.), *Handbook of Multivariate Experimental Psychology*. Chicago: Rand McNally.

MOTT, J. H. (1970). Reading interests of adolescents: a critical study of fifty years of research. (Unpublished Ph.D. thesis, University of Northern Colorado.)

RAVEN, J. C. (1965). *Guide to Using the Mill Hill Vocabulary Scale with the Progressive Matrices Scale*. London: H. K. Lewis.

SLATER, P. (1965). *The Principal Components of a Repertory Grid*. London: Vincent Andrews,

SLATER, P. (1972*a*). The measurement of consistency in repertory grids. *Br. J. Psychiat.* **121**. 45–51.

SLATER, P. (1972*b*). *Notes on Ingrid* 72. London: Institute of Psychiatry.

SNEDECOR, G. W. & COCHRAN, W. G. (1967). *Statistical Methods*, 6th ed. Ames, Iowa: Iowa State University Press.

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PREFERENCES FOR COMMON WORDS, UNCOMMON WORDS AND NON-WORDS BY CHILDREN AND YOUNG ADULTS

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In the first experiment, groups of 6–7-year-old, 10–11-year-old and 18–20-year-old subjects were presented with aesthetic choices between common words and unfamiliar non-words. All groups displayed a preference for real words but this tendency was significantly more pronounced in the intermediate age group than in either the younger or the older groups. In the second experiment, a group of 18–21-year-old subjects displayed a significant preference for uncommon words over common words, while groups of 7-year-old and 9–10-year-old subjects overwhelmingly preferred the common words. These results indicate an inverted-U function relating familiarity and liking, and this provides a parsimonious interpretation of the preferences of subjects of all ages.

It is well known that liking for a new tune or face often increases with time; it is also commonplace that we sometimes 'tire' of things very familiar. A great deal of empirical evidence of the former phenomenon has been found since Zajonc's (1968) revival of interest in the attitudinal effects of 'mere exposure', while the latter has been much less frequently reported in the experimental literature. Taken together, these two phenomena are more formally described as an inverted-U function relating familiarity and attraction. Crandall *et al.* (1973) have recently surveyed the relevant studies bearing on this hypothesized function; they have also examined factors other than exposure which influence familiarity.

An examination of these factors – complexity and discriminability of stimuli – would suggest that the inverted-U function might apply to children's liking for such things as letters of the alphabet and simple words. In a recent developmental study, Sluckin *et al.* (1973) found some evidence for a rise followed by a decline in liking for letters.

The problems of accounting for people's likes and dislikes and showing how they develop are obviously important and interesting. If, as the evidence suggests, familiarity is implicated in some way, it seems worthwhile to clarify its function. Intuition or 'common sense' does not, however, take us very far in this field: although the experiments described below involved preferences among very simple stimuli, prominent and unmistakable differences were found between the likes and dislikes of subjects of different ages which could not be predicted in advance on the basis of conventional wisdom and which beg for an explanation.

The first experiment to be described here examined preferences for common one-syllable words and completely unfamiliar non-words among subjects in three age groups. The second experiment investigated preferences for common and relatively uncommon two-syllable words again in three different age groups.

What distinguishes these studies from previous experiments in this field is not only their developmental interest but also the manner in which familiarity is manipulated. Instead of first being deliberately familiarized with stimuli, the subjects in our study are tested on stimuli which, for cultural reasons, are normally increasingly familiar with age; this method permits the investigation of material of extreme familiarity in addition to that of complete unfamiliarity.

EXPERIMENT I

Method

Subjects

The youngest of the three groups of subjects comprised 15 6–7-year-old pupils, and the intermediate group 15 10–11-year-old pupils of a primary school in Northamptonshire. The oldest group comprised 17 18–20-year-old students enrolled for a degree in combined studies at Leicester University.

Stimuli

The stimuli were all selected from Archer's (1960) list of all possible CVC trigrams. The eight real words chosen were those likely to be most familiar to all the subjects, and containing as great a variety as possible; in particular, no vowel occurs more than twice in the list, but each is represented at least once. The words finally selected were BAG, TAP, LEG, PEN, LID, DOT, JUG and CUP.

The eight non-words were selected from those of lowest possible association value (up to 27 per cent in Archer's study) but looking and sounding as word-like as possible; trigrams ending in J and F were, for example, rejected. The final list contains trigrams which, although very nonsensical according to Archer's findings, could each be transformed into a common word by changing the initial or final consonant. Care was taken to ensure that each vowel was represented with the same frequency as in the list of real words. The eight non-words finally selected were YAD, VAB, FEP, KEB, MIB, JOM, VUD and CUG.

Procedure

Every possible combination of a real and a non-word was printed in large lower-case letters on a separate card, once with the real word on the left and once in the reverse order, resulting in 128 test cards in all.

The school children were tested individually and were told that what was being investigated was 'what children of your age like'. The detailed instructions and procedure were similar to those described by Sluckin *et al.* (1973). Two trial cards similar to those used in the actual testing were presented, and the experimenter did not proceed until he was satisfied that the subject understood that what was required of him were evaluative preferences. The subject was then presented with each card in turn from the thoroughly shuffled deck and the chosen alternatives were recorded. The university students' preferences were recorded in essentially the same way, except that they were tested as a group, each subject recording his or her own choices separately.

Results

The frequency with which real and non-words were chosen by the subjects in the three age groups is shown in Table 1. It is clear that subjects in all age groups markedly preferred real to non-words; in fact, only two subjects, one in the youngest group and one in the oldest group, chose non-words more frequently than real words. The preference for words is highly significant in each of the three groups: a Wilcoxon test yielded $T = 1$ ($P < 0{\cdot}01$) for the youngest group, $T = 0$ ($P < 0{\cdot}01$) for the intermediate group, and $T = 3$ ($P < 0{\cdot}01$) for the oldest group.

The general demeanour and spontaneous comments of the subjects left little doubt that both the children and the adults were choosing according to their aesthetic preferences. When interviewed informally after the testing procedure, typical comments made by the youngest subjects were: 'I didn't think much of JOM' and 'DOT

Table 1. *Frequency of preferences for real words and non-words by subjects of different ages* ($N = 47$)

	6–7-year-olds ($n = 15$)		10–11-year-olds ($n = 15$)		18–20-year-olds ($n = 17$)	
	Real word	Non-word	Real word	Non-word	Real word	Non-word
	62	66	105	23	52	76
	72	56	112	16	69	59
	74	54	117	11	72	56
	76	52	126	2	81	47
	77	51	126	2	91	37
	82	46	128	0	93	35
	88	40	128	0	97	31
	106	22	128	0	99	29
	111	17	128	0	101	27
	117	11	128	0	102	26
	122	6	128	0	103	25
	123	5	128	0	105	23
	128	0	128	0	111	17
	128	0	128	0	115	13
	128	0	128	0	116	12
					128	0
					128	0
Median	106	22	128	0	101	27

was the nicest'. The older subjects typically remarked that 'CUG was ugly: I didn't like it at all' and 'LID was rather attractive'.

A further inspection of Table 1 suggests that the preference for real words was more pronounced in the intermediate age group than in either the younger or the older groups. It is striking that no fewer than two-thirds of the intermediate-aged subjects chose the real words in every single case, while few of the subjects in the other groups displayed such a consistent preference. A Kruskal–Wallis one-way analysis of variance revealed that the difference between the three groups was highly significant ($H = 16{\cdot}04$; $P < 0{\cdot}001$). Further analysis by means of the Mann–Whitney test showed that the intermediate-aged subjects' preference for words was significantly greater than that of either the younger group ($U = 40{\cdot}5$; $P < 0{\cdot}02$, two-tailed) or the older group ($U = 25{\cdot}5$; $P < 0{\cdot}002$, two-tailed), but that there was no significant difference between the youngest and the oldest groups ($U = 105$, n.s.).

EXPERIMENT II

Method

Subjects

The subjects were 20 7-year-old pupils and 20 9–10-year-old pupils of a primary school in Northamptonshire and 20 18–21-year-old students enrolled for a variety of courses at Leicester University.

The stimuli were six very common and six relatively uncommon two-syllable words roughly matched for meaning. None of the words had strongly evaluative connotations. The common words were APPLE, WINDOW, TRUMPET, BOTTLE, RABBIT and TEACHER. The uncommon words were GUAVA, CORNICE, CORNET, CARAFE, WOMBAT and MENTOR.

Table 2. *Frequency of preferences for common and uncommon words by subjects of different ages* ($N = 60$)

	7-year-olds ($n = 20$)		9–10-year-olds ($n = 20$)		18–21-year-olds ($n = 20$)	
	Common	Uncommon	Common	Uncommon	Common	Uncommon
	14	58	19	53	6	66
	36	36	27	45	8	64
	37	35	28	44	10	62
	39	33	28	44	12	60
	41	31	33	39	12	60
	41	31	33	39	17	55
	43	29	42	30	19	53
	43	29	44	28	22	50
	48	24	45	27	25	47
	52	20	46	26	27	45
	52	20	46	26	30	42
	52	20	47	25	30	42
	53	19	52	20	33	39
	53	19	57	15	35	37
	53	19	57	15	36	36
	55	17	58	14	36	36
	61	11	58	14	37	35
	62	10	60	12	38	34
	65	7	65	7	39	33
	70	2	72	0	40	32
Median	52	20	46	26	28·5	43·5

Procedure

The procedure adopted was essentially similar to that in Expt. I described above. As in Expt. I, all possible combinations of the two classes of stimuli were presented to the subjects on cards, and great care was taken to ensure that all subjects understood what was required of them. The only difference in procedure was that subjects from all age groups were tested in groups of six or seven and recorded their own preferences on forms provided by the experimenter.

Results

The frequency with which common and uncommon words were chosen by the subjects in the three age groups is shown in Table 2. It can be clearly seen that both the 7-year-old and the 9–10-year-old subjects markedly preferred common to uncommon words. Only one of the 7-year-old subjects and six of the 9–10-year-old subjects chose a majority of uncommon words. Among the 18–21-year-old subjects, on the other hand, a clear preference for uncommon words is evident, 14 out of 20 of these subjects choosing a majority of uncommon words.

The preference for common words in the two younger groups, and for uncommon words in the older group, is highly significant in each case: a Wilcoxon test yielded $T = 15$ ($P < 0{\cdot}01$), $T = 33{\cdot}5$ ($P < 0{\cdot}01$) and $T = 15$ ($P < 0{\cdot}01$), respectively.

In order to determine whether the difference between the choices of the three groups was significant, a Kruskal–Wallis one-way analysis of variance was first performed, and this yielded $H = 24{\cdot}998$ ($P < 0{\cdot}001$). Subsequent Mann–Whitney U tests revealed that the difference between the two younger groups was not significant ($U = 181$), but that the choices of the 18–20-year-old subjects were significantly

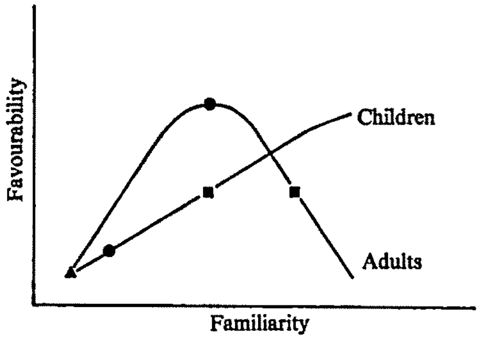

Fig. 1. ▲, Non-words; ●, uncommon words; ■, common words. (See explanation in text.)

different from those of both the 7-year-old subjects ($U = 25$; $P < 0{\cdot}002$, two-tailed) and the 9–10-year-old subjects ($U = 59$; $P < 0{\cdot}002$, two-tailed).

Discussion

The fact that in Expt. I all three groups strongly preferred words to non-words is consistent with the findings of Zajonc (1968), Harrison (1968) and subsequent studies which have suggested that familiarity tends to increase liking, since it can be assumed that the words used in this study were much more familiar to all the subjects than the non-words. Using a wide variety of stimuli, Zajonc, Harrison and their colleagues have almost always found a positive, monotonic relationship between familiarity and liking.

The preference for words in Expt. I was, however, significantly more pronounced in the intermediate age group than in either the younger or older groups. This points to a non-monotonic inverted-U relationship between familiarity and liking, if familiarity with common words is assumed to be directly related to age. While not unique (see Crandall *et al.*, 1973, for a review of relevant studies), this finding is unusual in the literature on exposure and attraction. One possible reason is that most previous experimental research has relied on the repeated exposure of initially unfamiliar stimuli in the laboratory to increase familiarity; and such a methodology is unlikely to have generated levels of familiarity in the subjects comparable with the degree of familiarity with common words of the young adults in the present study. Thus it is possible that only the rising part of the inverted-U function has been sampled in much of the previous research.

The results for the children in Expt. II again confirm the view that favourability increases with familiarity, since with the exception of CORNET the uncommon words were unfamiliar or unknown to the children. However, the young adult subjects greatly preferred the relatively unfamiliar words to the everyday ones. This gives strong support to the view that excessive familiarity tends to diminish liking. Taking into account the findings of both our experiments concerning word preferences of adults, we may infer that when a word is totally unfamiliar it is little liked, when it is known but is still rather strange then it is liked maximally, and when a word is highly familiar then it is not much liked – a clear inverted-U relationship between familiarity and liking.

It is obvious that familiarity is only one of the factors, and often not one of prime importance, that are behind the subjects' likes and dislikes of words; the affective meaning of words, their complexity, etc. may crucially influence our aesthetic preferences. Nevertheless, following on Zajonc (1968) and subsequent work, it now appears to be possible to elucidate the role of mere exposure in influencing liking in relation to age. Fig. 1 speculates about the nature of the relationship between familiarity and favourability in young adults and school children. Non-words are approximately equally low for adults and children on both familiarity and favourability. Uncommon words are much less familiar to children than to adults, and are much more liked by the adults. Common words are quite familiar to children and highly familiar to adults; they are liked by both groups, but lie on the ascending part of the inverted-U for the children and on the descending part for the adults. The proposed relationship between familiarity, favourability and age, as set out in Fig. 1, may suggest further empirical studies to test its correspondence with reality.

REFERENCES

ARCHER, E. J. (1960). A re-evaluation of the meaningfulness of all possible CVC trigrams. *Psychol. Monogr.* **74**, no. 10. (Whole no. 497.)

CRANDALL, J. E., MONTGOMERY, V. E. & REES, W. W. (1973). 'Mere' exposure *v.* familiarity, with implications for response competition and expectancy arousal hypothesis. *J. gen. Psychol.* **88**, 105–120.

HARRISON, A. A. (1968). Response competition, frequency, exploratory behaviour, and liking. *J. Person. soc. Psychol.* **9**, 363–368.

SLUCKIN, W., MILLER, L. B. & FRANKLIN, H. (1973). The influence of stimulus familiarity/novelty on children's expressed preferences. *Br. J. Psychol.* **64**, 563–567.

ZAJONC, R. B. (1968). Attitudinal effects of mere exposure. *J. Person. soc. Psychol.* Monogr. Suppl. **9**, 1–27.

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DEGREES OF NOVELTY AND THEIR EFFECTS ON CHILDREN'S ATTENTION AND PREFERENCE

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Two experiments investigated the question of whether those attributes of stimuli effective in eliciting attention are also effective in motivating choice. In the first experiment children of 4 and 6 years were given the opportunity to view pictures varying in novelty. They could choose the type of picture they wished to view and view as long as they wished. They also stated their preference. Novelty influenced attention and expressed preference but not choice. In the second experiment fewer stimuli were used in the familiarization phase, thereby increasing tedium, whereupon choice was also found to be influenced. The results are considered in the context of other studies which have related exposure to attention and preference.

There are two bodies of empirical evidence which are somewhat at variance with each other concerning the effects of familiarity upon preference. On the one hand, there are the studies of Cantor and his colleagues (Cantor & Cantor, 1964 *a*, *b*, 1966; Cantor, 1968; Cantor & Kubose, 1969), of Faw & Pien (1971) and of Siebold (1972) who found that repeated exposure of stimuli reduced liking for them relative to novel stimuli. On the other hand, there is the work of Zajonc and his colleagues (Zajonc, 1968; Harrison, 1968; Zajonc & Rajecki, 1969; Harrison & Zajonc, 1970; Zajonc *et al.*, 1968, 1972), which showed that 'mere exposure' to stimuli was sufficient to enhance preference for them. Although the Cantor studies were concerned with children and the Zajonc studies with adults, no specifically developmental processes have been, or indeed can be, adduced to explain the discrepancy.

Moreover, two recent studies appear to question the novelty-liking effect even in children. Freeman (1972), for instance, found a liking for the familiar amongst 5- and 6-year-olds, and Sluckin *et al.* (1973), on the basis of their results, suggested an inverted-U relationship between exposure and liking. Idiosyncracies of procedure and method, however, limit the generalizations which may be drawn from the results of these two studies. Nevertheless, Sluckin's point that 'more factual information is needed concerning developmental changes of aesthetic judgement' (p. 567) is well taken. The present experiment attempts to provide further information of this kind, and in doing so to assess the effects of different degrees of novelty upon preference, using two dependent measures of preference. In view of Harrison's (1968) demonstration of an inverse relationship between viewing time and affective ratings, we also took the opportunity to examine the effect of degrees of novelty. The procedure adopted enabled measures of visual attention, expressed preference and choice to be independently obtained.

EXPERIMENT I

Method

Subjects

There were 24 children, 12 of whom were 6-year-olds (mean age 6 years 6 months) and 12 4-year-olds (mean age 4 years 7 months), with equal numbers of boys and girls in each age-group. They were attending a local infant school and nursery school respectively.

Stimuli and apparatus

Twenty-four coloured pictures from children's books and magazines were photographed and made into slides. Eight of these were used in the familiarization procedure. The eight pictures were arbitrarily divided into four pairs, any one subject being shown only two pairs – one as the familiar (F) and one as the semi-novel (SN). Equal numbers of children saw each pair as F and SN.

The stimuli were projected in three windows set in a triangular arrangement in a console. To view a picture the window had to be opened and held open, since a spring on each window ensured that when the subject was not holding the window open firmly it would slam shut. Microswitches on the windows were connected to three channels of a Rustrak event-recorder so that the durations of the window-openings were automatically recorded.

The 16 novel (N) stimuli were projected in one window in a different random order for each subject; the two SN stimuli were projected alternately in the second window, and the two F stimuli in the third. The positions of the F, SN and N windows were counterbalanced over the subjects in each age group.

Procedure

Each child was taken individually into the room, seated at a low table, and asked if he would like to look at some pictures. The F and SN stimuli (the original pictures mounted on white cards) were then placed in front of him in the following order, each for 15 sec.: F_1, F_2, SN_1, F_2, F_1, SN_2, F_1, F_2, with an interval of 10 sec. between presentations. Thus the two F's were exposed for 45 sec. each, while the SN were exposed for 15 sec. each. During this presentation, attempts were made to draw the child's attention to all features of the picture. After this familiarization phase there was a pause of 2 min. during which the child was engaged in conversation. He was then asked if he would play with a toy-machine in which he could see more pictures and tell us what he thought of it.

Test phase. Each child was shown how the windows could be opened to view illuminated pictures, and was told he could open any window he wished and look at the picture for as long as he liked. He was told he could play with the toy for as long as he wished while the experimenter collected some papers together; the latter remained in an adjoining cubicle while in partial sight of the child. If the child paused after one or two responses, the instructions were repeated. At the end of 5 min., or when the child had ceased responding, whichever occurred earlier, the experimenter went in and said, 'You can have a look at just one more window now'. The child was then asked which window he liked the best and why. Finally, he was asked what he thought of the toy-machine.

Thus measures obtained were as follows: (1) *attention* – the total time spent in viewing each window; (2) *preference* – the explicit statement of which window was best liked; (3) *choice* – (*a*) the window most frequently opened, and (*b*) the final window chosen for viewing, i.e. after the instruction '...just one more window now'. The second choice measure was intended to be a more immediate, even though a weaker, measure than the first.

Results

All the children showed great interest in the apparatus and responded with enthusiasm, none terminating the experiment before they were requested to do so. The results will be discussed under the headings of the three response categories.

Attention. The amount of time viewing pictures in each of the three windows is shown in Table 1. Since the viewing scores for the three windows were not independent of each other, age, sex and habituation effects were examined by carrying out age (2) × sex (2) × time (5) analyses of variance on the viewing times for each window (F, SN and N), the time factor consisting of the scores for each successive minute. None of the factors or interactions was significant. The data were then collapsed over age, sex and time and a Friedman analysis of variance

Table 1. *Mean total viewing times of pictures varying in novelty and the number of window openings (responses) to view the different types of picture*

	Familiar	Semi-novel	Novel
(*a*) Experiment I			
Mean viewing times (sec.)	47·02	48·67	65·21
Mean number of window openings	9·13	9·70	10·28
(*b*) Experiment II			
Mean viewing times (sec.)	31·05	37·53	76·52
Mean number of window openings	7·12	7·64	11·88

Table 2. *Number of children who 'most liked' windows in which they saw Familiar, Semi-novel or Novel pictures*

	Familiar	Semi-novel	Novel
4 yr.	4	4	4
6 yr.	0	1	11

carried out, which showed the novelty effect to be highly significant ($\chi_r^2 = 16{\cdot}19$; d.f. = 2; $P < 0{\cdot}001$). Children looked longest at the completely novel pictures, less at the semi-novel and least at the familiar. Analysis of trend (Ferguson, 1965) showed that only the monotonic component was significant ($z = 3{\cdot}73$; $P < 0{\cdot}001$).

Preference. Inspection of the expressed preferences of the children gave no indication of effects attributable to sex, but as can be seen from Table 2, the 6-year-olds unequivocally expressed preference for the N window, whereas the preferences of the 4-year-olds were equally distributed over the three types of window. The performance of the younger children here was very similar to that of the under-fives in an earlier study (Aitken & Hutt, 1975), suggesting that these children have difficulty in interpreting the term 'like'. Acquisition of the meaning of this term and its dependence on perceptual features are explored elsewhere (Hutt *et al.*, 1975).

Choice. (*a*) As seen from Table 1*a*, there was a slight tendency for the N window to be opened more frequently than the SN and the SN more than the F. Once again, age (2) × sex (2) × time (5) analyses of variance were carried out on the number of openings of each window, and in the absence of significant effects, the data collapsed over age, sex and time. A Friedman analysis of variance revealed that the novelty effects failed to reach significance ($\chi^2 = 2{\cdot}77$). A less conservative analysis of trend (Ferguson, 1965), however, showed the monotonic component to be moderately significant ($z = 1{\cdot}96$; $P = 0{\cdot}05$).

(*b*) In terms of the last window chosen, some effect of novelty was discernible in that four chose F, eight chose SN and 12 chose N, but these frequencies did not differ significantly ($\chi^2 = 4{\cdot}00$).

It is likely that, to influence choice, preference requires to be fairly strong, whereas even a slight preference may be reflected in a verbal expression. Preference may be strengthened by inducing tedium. As Berlyne (1970) argues, stimulus repetition has two effects: positive habituation, resulting from the reduction of uncertainty, ambiguity, curiosity, etc., and the induction of tedium. The negative affect associated with such tedium may effectively enhance preference for the novel stimuli, and thereby influence choice. Since it may be argued that the conditions of this experiment, in using four different stimuli in the familiarization procedure, were not conducive to the production of tedium, a further experiment was conducted to specifically examine the relation of tedium to choice.

EXPERIMENT II

Method

Twelve 6-year-olds (mean age 6 years 4 months) from a local infant school were the subjects. The apparatus, stimuli and procedure were essentially the same as in Expt. I, with the following exception: only two stimuli (one F and one SN) were used during the familiarization phase and presented in the sequence F, F, SN, F, F, each exposure now being 10 sec. Thus F was presented for a total of 40 sec.

Results

Attention. The viewing times for the F, SN and N pictures are shown in Table 1*b*. A Friedman analysis of variance showed that the novelty effect was again highly significant ($\chi^2 = 12{\cdot}66$; d.f. $= 2$; $P < 0{\cdot}01$). This effect however, was entirely contributed by the viewing scores of the N stimuli, since Wilcoxon matched-pairs signed-ranks test showed the difference between F and SN to be non-significant.

Preference. All 12 subjects expressed preference for the N window.

Choice. (*a*) As Fig. 2 indicates, the N window was opened far more frequently than SN or F, the novelty effect being significant (Friedman's $\chi^2 = 7{\cdot}93$; d.f. $= 2$; $P < 0{\cdot}02$).

(*b*) The effect of novelty was again evident with respect to the last window chosen: two chose F, one chose SN and nine chose N.

DISCUSSION

The effectiveness of novelty in eliciting attention was once again demonstrated by these experiments. Most unexpected, however, was the finding that even a single prior exposure should so substantially reduce attention to a stimulus (Expt. II). Moreover, it seems that it is not simply the *absolute* duration of prior exposure that is the effective factor in reducing attention but that the *relative* amount of change or variety inherent in the test procedure is also a salient factor. Thus, although the pictures were viewed for briefer periods during the familiarization phase of Expt. II, they nevertheless elicited less attention during the test phase.

The reduction in stimulus variety also had the effect of influencing the 'choice' measure of preference, in so far that the N window was opened much more frequently than the other two. The children's comments during Expt. II supported this interpretation: frequently they would remark, 'Oh, its the same one again' or 'It hasn't changed'. Thus the fact that the pictures did not change in the F and SN windows, irrespective of their degree of familiarity, contributed towards enhancing tedium, thereby influencing choice. It is hoped that subsequent experiments will be able to separate the effects of lack of stimulus change from those of familiarity *per se*.

With respect to expressed preference, the effect of the novelty–familiarity dimension was much the same in Expt. II as it had been in Expt. I. Thus it appears that this measure of preference is more readily influenced by novelty than is the choice measure. It may be that the enactment of an instrumental response, like opening a window, has some intrinsic incentive value which reduces the force of the independent variable, viz. novelty.

Having demonstrated that prior exposure, even once, decreases preference, how then do we reconcile our results with those of Zajonc and his colleagues? Examination of these studies reveals that the demand characteristics of the Zajonc studies are very different from ours and Cantor's; these demands derive chiefly from the stimulus material (Chinese ideographs and nonsense trigrams) and the brevity of the exposures.* Both these factors militate to make the subject feel that recognition and identification of the stimuli are important. When more meaningful stimuli were used (Zajonc *et al.*, 1972) or when longer exposures were given (Hamid, 1973) the linear relationship between exposure and liking was not obtained. Presumably with stimuli that are already identifiable and adequately exposed the inverse relationship between exposure and liking is more likely to be obtained, as Faw & Pien (1971) and Siebold (1972) found. The present experiments reinforce the view that although the relationship between novelty and attention is fairly unequivocal, that between novelty and preference is highly idiosyncratic.

Moreover, as these experiments, and others manipulating different attributes of stimuli (Aitken & Hutt, 1975), have shown, generalizations regarding these attributes and preference are wholly dependent on the measure of preference considered: expressed preference is most strongly influenced by the independent variable, a final-option ('last chance') measure less so, and a choice measure involving an instrumental response least of all.

A plausible interpretation of these results is that expressed preference is determined to a large extent by the attention value of stimuli, whereas choice appears to be dependent in part on the tedium value of the alternatives.

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REFERENCES

AITKEN, P. P. & HUTT, C. (1975). The effects of stimulus incongruity upon children's attention, choice and expressed preference. *J. exp. child Psychol.* **19**, 79–87.

BERLYNE, D. E. (1970). Novelty, complexity and hedonic value. *Percept. Psychophys.* **8**, 279–286.

CANTOR, G. N. (1968). Children's 'like–dislike' ratings of familiarized and non-familiarized stimuli. *J. exp. child Psychol.* **6**, 651–657.

CANTOR, G. N. & CANTOR, J. H. (1964*a*). Effects of conditioned-stimulus familiarization on instrumental learning in children. *J. exp. child Psychol.* **1**, 71–78.

CANTOR, G. N. & KUBOSE, S. K. (1969). Preschool children's rating of familiarized and non-familiarized visual stimuli. *J. exp. child Psychol.* **8**, 74–81.

CANTOR, J. H. & CANTOR, G. N. (1964*b*). Observing behavior in children as a function of stimulus novelty. *Child Dev.* **35**, 119–128.

CANTOR, J. H. & CANTOR, G. N. (1966). Functions relating children's observing behaviour to amount and recency of stimulus familiarization. *J. exp. Psychol.* **72**, 859–863.

FAW, T. T. & PIEN, D. (1971). The influence of stimulus exposure on rated preferences: effects of age, patterns of exposure, and stimulus meaningfulness. *J. exp. child Psychol.* **11**, 339–346.

FERGUSON, G. A. (1965). *Nonparametric Trend Analysis*. Montreal: McGill University Press.

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* During revision of this paper a review article (Stang, 1974) on this topic appeared in which the author concluded that the nature of the stimulus material was an important factor in these studies.

FREEMAN, N. H. (1972). The influence of short-term novelty on affective assertions in young children. *Br. J. Psychol.* **63**, 547–553.

HAMID, P. N. (1973). Exposure frequency and stimulus preference. *Br. J. Psychol.* **64**, 569–577.

HARRISON, A. A. (1968). Response competition, frequency, exploratory behaviour and liking. *J. Person. soc. Psychol.* **9**, 363–368.

HARRISON, A. A. & ZAJONC, R. B. (1970). The effects of frequency and duration of exposure on response competition and affective ratings. *J. Psychol.* **75**, 163–170.

HUTT, C., FORREST, B. & NEWTON, J. (1975). The visual preferences of children. *J. child Psychol. Psychiat.* (in the Press).

SIEBOLD, J. R. (1972). Children's rating responses as related to amount and recency of stimulus familiarization and stimulus complexity. *J. exp. child Psychol.* **14**, 257–264.

SLUCKIN, W., MILLER, L. B. & FRANKLIN, H. (1973). The influence of stimulus familiarity/novelty on children's expressed preferences. *Br. J. Psychol.* **64**, 563–567.

STANG, D. J. (1974). Methodological factors in mere exposure research. *Psychol. Bull.* **81**, 1014–1025.

ZAJONC, R. B. (1968). Attitudinal effects of mere exposure. *J. Person. soc. Psychol.* Monogr. Suppl. **9** (2, pt. 2), 1–27.

ZAJONC, R. B. & RAJECKI, D. W. (1969). Exposure and affect: a field experiment. *Psychon. Sci.* **17**, 216–217.

ZAJONC, R. B., SHAVER, P., TAVRIS, C. & VAN KREVELD, D. (1972). Exposure, satiation and stimulus discriminability. *J. Person. soc. Psychol.* **21**, 270–280.

ZAJONC, R. B., SWAP, W. C., HARRISON, A. A. & ROBERTS, P. (1968). Limiting conditions of the exposure effect: satiation and relativity. *J. Person. soc. Psychol.* **18**, 384–391.

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THE EFFECTS OF CHRONOLOGICAL AGE ON THE RELATIONSHIP OF INTELLIGENCE AND ACADEMIC ACHIEVEMENT WITH EXTRAVERSION AND NEUROTICISM

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This study examines the possibility that the relationships of intelligence and academic achievement with both extraversion and neuroticism may vary with chronological age. A sample of 741 students in the 15–19 age group worked through a battery of four tests, two of which were tests of intelligence and two tests of achievement in chemistry. The nature of the relationships between performance on these tests and measures of extraversion, neuroticism and chronological age were then expressed as multiple regression equations. The results showed a consistently different pattern of significant relationships for both extraversion and neuroticism with the achievement tests on the one hand and with the intelligence tests on the other. Only the performance on the two achievement tests depended upon an interaction between extraversion and chronological age. There were no such interactions involving neuroticism and chronological age.

The possible existence of interactions involving chronological age in determining the relationships of both intelligence and academic achievement with extraversion and neuroticism has been indicated by a number of previous studies. For example, some results reported by Eysenck (1965) for students in the age range 11–16 suggest that the correlations of intelligence with both extraversion and neuroticism change from zero to negative over the age of 13. However, the correlations with the older students were obtained on such small numbers of students that no definitive conclusions can be drawn. There are also numerous studies in which different tests of academic achievement have been administered to different students over the age range 5–18 +, and which report the correlations of achievement with both extraversion and neuroticism as varying between −0·3 and +0·3 (Entwistle, 1972). In reviewing these experiments in some detail Entwistle concludes that the correlations may well vary systematically with age. In the case of extraversion he suggests that the relationships might change from positive to negative as age increases. Furthermore he feels that the change in sign could occur between the ages 12 and 15. Anthony (1973) has offered a tentative explanation of such an effect based upon the observed inverted-U type regression of extraversion on chronological age, in which the maximum of the curve appears to be somewhere in the age-range 12–15. For relationships with neuroticism the existence of any trend is less easily discerned than it is in the case of extraversion. However, Entwistle suggests that correlations might increase as chronological age increases.

Interesting though these changing relationships are, most of the experiments are insufficient in themselves to establish definitively the interactions involving age. In particular, there appear to be no studies in which the same achievement tests were administered to students of different ages. As a result the effects of changing chronological age are superimposed on the effects of changing the nature of the tests used as dependent variables. It is obvious, therefore, that no unequivocal conclusions can be drawn from such a set of experiments. Moreover, there may be no need to invoke an interaction with chronological age, because it can be shown that the observed changes in the correlation coefficients may well be due entirely to very small concomitant changes in the nature of the test.

Thus if two tests, X and Y measure different aspects of academic achievement, the extent to which the two tests are different, may be reflected in the extent to which the correlation coefficient, r_{XY}, differs from unity. If P now represents one of the two personality tests, the correlation coefficients, r_{XP} and r_{YP}, may differ from each other considerably when r_{XY} differs from unity. Moreover, there is a mathematical relationship which describes by how much r_{YP} may differ from r_{XP} for particular values of r_{XY} and r_{XP} (Stanley & Wang, 1969; Glass & Collins, 1970). The limits of r_{YP} are:

$$r_{XP} \cdot r_{XY} \pm \sqrt{[(1-r^2_{XP})(1-r^2_{XY})]}. \tag{1}$$

Equation (1) may then be used to calculate the limits of r_{YP} for any combination of r_{XP} and r_{XY}. Now, as mentioned previously, the correlation coefficients between different tests of academic achievement and each of these personality variables usually range between $-0{\cdot}3$ and $+0{\cdot}3$. Also the correlation coefficients, r_{XY}, between different achievement tests tend to be positive and cover the whole range from 0·1 to say, 0·9. Hence, by substituting these values in equation (1), it is easily demonstrated that for each one of the various combinations of values for r_{XP} and r_{XY}, r_{YP} can range from being positive to being negative. Furthermore when r_{XY} is as high as 0·8, r_{YP} can be expected to be anywhere in the range $-0{\cdot}3$ to $+0{\cdot}3$. Thus as a result of making quite small differences between tests X and Y, r_{XP} and r_{YP} may be made to differ in a manner which corresponds exactly to the manner in which the correlations of academic achievement with these personality variables are found to vary over all the previous experiments. Therefore, it is reasonable to conclude that the observed changes in these latter relationships could well be due solely to changes in the nature of the tests of academic achievement.

However, this argument is not being presented in an attempt to deny the existence of interactions with chronological age. It is presented merely to emphasize that evidence from previous experiments in which students of different ages have worked through different tests, cannot be interpreted unequivocally in terms of interactions involving chronological age. A more clear-cut demonstration of an interaction involving chronological age has been reported by Leith & Davis (1972) in a study concerned with investigating the relationship between neuroticism and the amount learned from a self-instructional programme by 12 and 13 year olds. Leith & Davis observed a main effect corresponding to a significant inverted-U relationship, which was modified, on increasing the chronological age of the students, by the maximum of the curve moving towards higher levels of neuroticism.

In conclusion, there is as yet very little evidence which demonstrates unequivocally that the nature of the relationships of intelligence and academic achievement, with both extraversion and neuroticism varies with chronological age. The present experiment was therefore intended to produce results which allow more definitive conclusions to be drawn.

Table 1. *Distribution of the subjects among the different age groups and types of academic institution*

Age group (years)	Schools	Colleges of education	Universities
15	97	0	0
16	247	0	0
17	178	0	12
18	11	0	141
19+	0	21	34
All groups	533	21	187

METHOD

The basic plan was to administer a variety of tests of intelligence and academic achievement to a group of students covering a range of ages and to investigate the nature of the relationships between performance on these tests with measures of extraversion, neuroticism, and chronological age. The relationships which were of particular interest were whether the regression of intelligence or academic achievement on each of extraversion and neuroticism is rectilinear or curvilinear, and whether there are significant interactions for each of these personality traits with chronological age.

Subjects

It is of course impracticable to attempt to administer the same tests of intelligence or academic achievement over the whole age range, 5–18 plus. It was therefore decided to concentrate on the 15–18 plus age range, where it is less difficult to devise tests which are suitable for use with all the students involved. In fact, all the subjects were students of chemistry in either secondary or tertiary institutions. The 25 secondary schools involved included comprehensive, grammar and public schools, drawn mainly from the Midlands. Each school was preparing the student for either 'O' or 'A' level chemistry examinations. From the higher education sector two colleges of education and three universities allowed all the first-year chemistry students who were potential candidates for honours degrees to participate. The number and distribution of the students among the different age groups and different types of institution are summarized in Table 1.

Tests

It was decided to use four tests as dependent variables. Two of them were to be tests of intelligence, whereas the other two were to be tests of achievement in chemistry. The intelligence tests were further subdivided into verbal and non-verbal intelligence, and the two achievement tests were also to cover different subject matter areas. In one case the test was to cover a very general area of subject matter and in the other a very specific area.

In choosing or constructing such intelligence and achievement tests, it is important, for the purpose of the present experiment, that they should be able to discriminate between students within each age group and between students over the whole age range. An ideal intelligence test here is AH5 (Heim, 1956) because each of the two parts (i.e. verbal and non-verbal) is specifically designed to discriminate well among students such as those in the sample. In the case of the achievement tests the students in the present sample could be considered to be at four distinct stages or standards of chemical achievement, i.e. fifth form, first-year sixth form, second-year sixth form and first year university or college of education. It follows that so far as content is concerned, the tests must sample a wide variety of topics including some which would normally be considered suitable for inclusion in examinations set at any of these levels. In the case of the test of general background knowledge one test was found to be suitable. It was 'High School Chemistry' 1965 (Part II) published by the American Chemical Society – National Science Teachers Association Cooperative Examinations Committee. As required, the items corresponding to each age group covered a wide range of subject matter. The test of specific background knowledge in chemistry was constructed specially for the experiment and was concerned with

Table 2. *Means and standard deviations of the various tests for the different age groups*

Group		Age (years)	Verbal intelligence	Non-verbal intelligence	General background knowledge	Specific background knowledge	Extraversion	Neuroticism
15-year-olds	Mean	15·4	15·5	20·1	14·7	14·8	15·5	11·4
(n = 97)	S.D.	0·25	3·9	5·0	6·5	9·1	3·8	4·0
16-year-olds	Mean	16·4	17·2	21·0	18·5	20·5	14·3	11·3
(n = 247)	S.D.	0·29	4·1	4·6	6·1	9·9	4·0	4·0
17-year-olds	Mean	17·4	17·5	20·8	21·1	20·6	14·7	11·5
(n = 190)	S.D.	0·27	3·8	4·8	6·7	10·5	3·8	4·4
18-year-olds	Mean	18·9	18·1	21·8	26·4	23·6	14·0	11·9
(n = 207)	S.D.	1·5	4·1	4·3	6·0	10·9	4·1	4·3
All age groups	Mean	17·2	17·3	21·0	20·8	20·7	14·3	11·6
n = 741	S.D.	1·4	4·1	4·7	7·4	10·6	4·0	4·2

a very restricted set of objectives. Very briefly, these objectives required the student firstly to identify graphs showing the correct trends in the magnitudes of various physical properties of binary compounds in certain parts of the Periodic Table of the Elements, and secondly to identify diagrams showing the three-dimensional structures which could be adopted by these substances in the solid state.

The Eysenck Personality Inventory (Eysenck & Eysenck, 1964) was chosen to measure both extraversion and neuroticism, even though, strictly speaking, it has been validated only with subjects comparable to the older students in the present sample. However, an investigation into the suitability of these two scales for 15 and 16-year-olds (Seddon, 1969) indicated that they are still quite acceptable for the present purpose.

Procedure

In each institution the intelligence and personality tests were administered in the same week, EPI always being done first, and the students took the two achievement tests some time later (i.e. 6–10 weeks). All of the testing in the various institutions was carried out in the same six-month period at the end of the calendar year.

RESULTS

Table 2 gives the means and standard deviations obtained by the various age groupings on each of the six tests.

A suitable method of analysis is multiple regression analysis, based upon an equation which relates scores, X_A, on each criterion task to the measures of extraversion, X_E, neuroticism, X_N, and chronological age, X_{CA}, in such a way that the nature of the terms included in this equation should be able to simulate the various curvilinear and interaction effects under investigation. The existence of each effect can then be investigated by testing the significance of the corresponding terms. In the event of the various curvilinear effects not appearing to be significant, it is obviously important to know whether the relationship is in fact rectilinear or just not significant at all. It was therefore decided to include terms corresponding to rectilinear effects as well as terms corresponding to curvilinear effects.

The actual form of the terms corresponding to rectilinear effects is standard and requires no further description. The terms corresponding to curvilinear effects must allow the complete curves to reach either a maximum – in accordance with the Yerkes–Dodson law, for example – or a minimum. The most convenient terms for this

Table 3. ***Results of the stepwise multiple regression analyses***

Variable	β-weights ($\times 10^2$)						
	(1) Verbal intelligence						
CA^*	11	11	11	11	11	−09	−17
E	—	−04	−05	−24	−24	−34	−36
N	—	—	−07	−07	−11	−11	−17
E^2	—	—	—	20	20	24	23
N^2	—	—	—	—	04	04	04
$CA \times E$	—	—	—	—	—	13	19
$CA \times N$	—	—	—	—	—	—	15
R ($\times 10^2$)	11	12	14	14	14	15	16
P	—	—	—	—	—	—	—
	(2) Non verbal intelligence						
CA^*	06	05	05	05	05	−08	−01
E	—	−05	−05	−14	−15	−26	−25
N	—	—	−04	−03	−20	−20	−18
E^2	—	—	—	09	10	14	14
N^2	—	—	—	—	17	17	18
$CA \times E$	—	—	—	—	—	15	13
$CA \times N$	—	—	—	—	—	—	−06
R ($\times 10^2$)	06	07	08	08	09	11	11
P	—	—	—	—	—	—	—
	(3) General background knowledge						
CA^*	40	39	39	40	40	04	13
E	—	−17	−18	−11	−11	−42	−41
N	—	—	−11	−11	−11	−13	−10
E^2	—	—	—	−07	−07	04	05
N^2	—	—	—	—	00	02	02
$CA \times E$	—	—	—	—	—	42	39
$CA \times N$	—	—	—	—	—	—	−07
R ($\times 10^2$)	40	43	45	45	45	47	47
P	1%	1%	1%	—	—	1%	—
	(4) Specific background knowledge						
CA^*	16	15	15	16	16	−13	−29
E	—	−13	−14	05	06	−19	−21
N	—	—	−11	−11	09	08	02
E^2	—	—	—	−20	−21	−12	−13
N^2	—	—	—	—	−20	−20	−20
$CA \times E$	—	—	—	—	—	34	40
$CA \times N$	—	—	—	—	—	—	14
R ($\times 10^2$)	16	20	23	23	24	27	27
P	—	1%	1%	—	—	1%	

* Ages are measured to the nearest quarter of a year

purpose are second order in the appropriate independent variables, e.g. X^2_E and X^2_N, with the sign and magnitude of the β-weights or partial regression weights indicating the direction and extent of curvature respectively. Finally the interactions are investigated by means of terms involving cross-products of raw-scores, e.g. $X_{CA}.X_E$ and $X_{CA}.X_N$. Hence the final multiple regression equation corresponds to equation (2):

$$\hat{X}_A = b_1.X_{CA} + b_2.X_E + b_3.X_N + b_4.X^2_E + b_5.X^2_N + b_6.X_{CA}.X_E + b_7.X_{CA}.X_N + \text{const.} \qquad (2)$$

Table 4. *Summary of the variables which turned out to be significant at the 1 per cent level*

	Dependent variables			
Terms concerned with	Verbal intelligence	Non-verbal intelligence	General background knowledge	Specific background knowledge
Extraversion	—	—	$E\,(-)$ $CA \times E\,(+)$	$E\,(-)$ $CA \times E\,(+)$
Neuroticism	—	—	$N\,(-)$	$N\,(-)$

The results of the multiple regression analysis for each of the four criterion variables are shown in Table 3, which for each dependent variable presents a whole series of multiple regression equations gradually building up to the final equation by the addition of a new term on moving from one equation to the next. In this matrix format the β-weights and values of multiple-R for any one equation in this stepwise procedure are presented in one column, and the independent variables themselves are given, in the order in which they were introdued, down the left-hand column of the matrix. The significance of each term is assessed by testing whether or not the predictability is significantly improved after adding that particular term (McNemar, 1969, p. 321). In cases where terms are significant at the 0·01 level, this fact is indicated at the foot of the appropriate column in Table 3.

A brief overall summary of the results is given in Table 4, where for each dependent variable the terms found to be significant in the multiple regression analysis are listed together with a plus or minus sign according to whether the sign of the respective β-weight is positive or negative.

DISCUSSION

Table 2 shows that for each of the tests used as dependent variables, all four age groups have substantial and comparable standard deviations. The mean scores for each age group also increase fairly steadily over the whole age range. The four dependent variables are therefore regarded as giving adequate discrimination of the students within and over the different age groups.

Table 4 shows that there were no significant interactions involving chronological age and either extraversion or neuroticism in determining the relationships with either of the two intelligence tests. However, in the case of the two achievement tests there are significant interactions which operate so as to increase the partial relationship between achievement and extraversion as chronological age increases. In contrast to the experiment of Leith & Davis (1972), the present experiment does not reveal the existence of an interaction between neuroticism and chronological age. However, neither does it reveal the existence of a curvilinear relationship between performance and neuroticism, and it is possible that both these differences between the present results and those of Leith & Davis are due to other effects–e.g. the different age range of the students.

In considering the changing pattern of significant terms for the various tests it will be noted that the two achievement tests have identical sets of significant terms in the multiple regression equations, and that the two intelligence tests have no significant terms in theirs. It will be noted also that the terms which are significant for the achievement tests involve both extraversion and neuroticism. The results are therefore in agreement with the hypothesis that the relationships of the dependent variables with both extraversion and neuroticism change with the nature of the dependent variable. However, it is evident that neither the differences between the two types of intelligence test nor the differences between the two types of achievement test were sufficiently great to give rise to significantly different relationships for the two types of intelligence test on the one hand or the two types of achievement test on the other.

In conclusion it is important to emphasise that all the findings of this study refer only to students within the age range 15–18 + and within very restricted levels of ability. It is therefore quite possible that for both intelligence and academic achievement, different results will be obtained for students belonging to different age and ability ranges. It is also necessary to recognize that, as students progress through these different levels of the educational system, they are required to survive a sequence of selection procedures which considerably reduce the number of students transferring from the pre-'O'-level classes to the sixth forms at the age of 16 or so, and from the sixth forms to the universities and colleges of education at the age of 17/18. As a result, in the present study the changes in the age groups are confounded with concomitant changes in other traits and abilities which may be important in determining a student's performance on tests of achievement or intelligence. It must be acknowledged, therefore, that the results do not necessarily refer directly to the intrinsic effects of chronological age. The results describe effects which may be only incidentally related to chronological age, and which may be observed only in groups of students taken from sectors of the educational system where there is a similar succession of selection procedures as age increases.

Nevertheless, bearing these limitations in mind, the study has demonstrated that for students, as they are presently found at each age level within these particular sectors of the British educational system, there is one subject-matter area where the relationship between academic achievement and extraversion changes as chronological age increases. However, there appear to be no such changes in the relationship between achievement in the same subject matter area and neuroticism or in the relationship between intelligence and either extraversion or neuroticism.

REFERENCES

ANTHONY, W. S. (1973). The development of extraversion, of ability and of the relation between them. *Br. J. educ. Psychol.* 43, 223–227.

ENTWISTLE, N. J. (1972). Personality and academic attainment. *Br. J. educ. Psychol.* 42, 137–151.

EYSENCK, H. J. & EYSENCK, S. B. G. (1964). *Manual of the Eysenck Personality Inventory.* London: University of London Press.

EYSENCK, S. B. G. (1965). *Manual of the Junior Eysenck Personality Inventory.* London: University of London Press.

GLASS, G. V. & COLLINS, J. R. (1970). Geometric proof of the restriction on the possible values of r_{XY} when r_{XZ} and r_{YZ} are fixed. *Educ. psychol. Measur.* 30, 37–39.

HEIM, A. W. (1956). *AH5 Group Test of High-Grade Intelligence.* Slough: National Foundation For Educational Research.

Br. J. Psychol. (1975), **66**, 4, *pp.* 501–511
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AN ANALYSIS OF THE ITEMS OF THE EYSENCK PERSONALITY INVENTORY

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An item analysis was carried out on Forms A ($n = 200$) and B ($n = 252$) of the Eysenck Personality Inventory (EPI) in order to establish how well the items perform. The method was, for each scale, to compare the mean total score of the people who said 'yes' to each item with the mean total score for the people who said 'no'. The differences between the means were tested for significance, and it was found that some items had no significant differences, some differed significantly on the expected scale, some on the expected scale as well as others, some better on scales other than the expected one, and some did not differ on the expected scale but did on another scale. Particularly useful was the discovery of items which when answered 'no' predict the neuroticism score, since such items are not included in the EPI. On the basis of the analysis, various tests could be constructed. Useful tests might contain a better balance of 'yes' and 'no' items, items which predict the total scores better than does the average item, or only the very best items to make up a very short form for the selection of extreme groups.

In this laboratory a number of experiments on aspects of personality inventories have been carried out. These have involved discovering the extent to which a person can simulate various types of personality when completing a questionnaire, and whether he can evade detection by a lie scale. Recently we have used multivariate analysis to establish the effectiveness of inventories when deciding to which of several groups an individual belongs, and have decided that inventories can be quite powerful diagnostic tools when used in this way. This work has been summarized (Power, 1975). More recently we have compared the diagnostic ability of a group of clinical psychologists given adequate normative data with the performance of discriminant function analysis, and have found that the psychologists perform, on average, almost as well as does discriminant analysis (Power *et al.*, 1975).

These studies have led us to consider aspects of the construction of personality inventories. The particular study (Power & MacRae, 1971) which led to the investigation reported here was concerned with the detectability of items in the Eysenck Personality Inventory (EPI) as being items of a certain type. We found that most items were readily detectable, and that this detectability was positively related to the magnitude of the factor loadings of the items, about 50 per cent of the variance in the task being explained by these loadings. We were concerned to note that the average factor loadings of the items differed from one scale to the other, that the loadings on one factor were more homogeneous than the loadings on the other, and that some of the items keyed as belonging to one factor had higher loadings on the other factor. Another aspect of the inventory which concerned us was the fact that some of the neuroticism items were considered by half or more of our subjects to belong to the lie scale. An example is 'Do you sometimes sulk?' An answer of 'yes' contributes a score of 1 to the neuroticism scale, but a clear majority of our subjects thought that saying 'no' was a lie. Given these points, we decided that it would be interesting to carry out an item analysis of the EPI. In carrying out the item analysis we accepted the basic assumption that the test, as a whole, was a valid measure, and we directed our efforts towards purifying the test and examining the properties of the items in detail.

METHOD

Form A of the EPI was completed in the usual fashion by 200 students at the beginning of an introductory course in psychology in 1972, and Form B was completed in a similar manner in 1973 by 252 students.

An item analysis was then carried out in which the response to each item was related to the total score on each of the three scales, Extraversion, Neuroticism and the Lie scale, for the appropriate form of the test. This was done by determining, for each of the 57 items in each form of the test in turn, the mean Extraversion, Neuroticism and Lie scores for subjects responding 'yes' and 'no' respectively on that item. Also the percentage of subjects answering 'yes' was computed. To ascertain whether the difference in the scores for the 'yes' and 'no' responders differed significantly, the difference between the 'yes' and 'no' responders on each of the scales was tested using Student's *t*.

RESULTS AND DISCUSSION

The means and standard deviations for the three scales of the EPI for the 200 Form A and 252 Form B subjects are shown in Table 1. With both the Extraversion and Lie scales, the means of the scores differ by about half of a standard deviation, although the Neuroticism means are quite similar to each other. As a consideration of the individual items will show, the two forms of the EPI are sufficiently different for differences of this magnitude to be unremarkable. Indeed, the student norms for Extraversion on Forms A and B of the EPI (Eysenck & Eysenck, 1964) differ by a similar amount.

The structure of the two forms of the EPI is shown in Table 2, together with the mean of the factor loadings for the items contributing to the scales (Eysenck *et al.*, 1969).

As has been stated already, the EPI contains three scales: Extraversion, Neuroticism and Lie. The 57 items in each form of the test comprise 24 scored as Neuroticism items, 24 scored as Extraversion items and nine scored as Lie items. The Neuroticism items are all of one type, as far as the scoring is concerned, in that a 'yes' answer is the neurotic response. Thus all the Neuroticism items are scored so that a 'yes' response adds 1 to the Neuroticism total for that subject. The Extraversion items are of two sorts. Fifteen of them when answered 'yes' are scored as adding 1 to the Extraversion total, and the remaining nine when answered 'no' are scored as adding 1 to the Extraversion total. These two sorts of Extraversion items are referred to here as E + and E −, the E + ones being those where the 'yes' answer is extraverted and the E − ones those in which the 'no' answer is extraverted. Similarly, there are two sorts of Lie items. There are three L + items, that is items where a 'yes' answer adds 1 to the total Lie score, and six L − items, in which a 'no' answer adds 1 to the total Lie score. Clearly, then, possible response bias is not controlled by the design of the test. A difficulty has arisen, for example, in interpreting the low *N* score of males with malignant disease: Huggan (1968) suggests that these individuals could be 'No-sayers', while Kissen (1966) interprets this finding in terms of low neuroticism.

Table 1. *Means and standard deviations of the three scales, Extraversion (E), Neuroticism (N) and Lying (L) for forms A and B of the EPI*

	Form A ($n = 200$)		Form B ($n = 252$)	
	Mean	S.D.	Mean	S.D.
E	12·4	4·2	14·4	3·7
N	11·4	4·3	11·6	4·3
L	2·0	1·4	1·4	1·3

Table 2. *Distribution of the 57 items in each of the two forms of the EPI with mean factor loadings for items in each scale*

(Neuroticism (N+), Extraversion 'yes' (E+), Extraversion 'no' (E−), Lying 'yes' (L+) and Lying 'no' (L−).)

Scale of items	Number of items per form per scale	Eysencks' factor loadings			
		Form A		Form B	
		F_1(N)	F_2(E)	F_1(N)	F_2(E)
N+	24	0·41	0·03	0·41	0·10
E+	15	0·00	0·39	−0·09	0·33
E−	9	0·07	−0·26	0·11	−0·24
L+	3	—	—	—	—
L−	6	—	—	—	—

The factor loadings of the E+ items are much higher than those of the E− items. It can also be seen that the factor loadings of the N+ items on Neuroticism are higher than the factor loadings of either of the two types of Extraversion items. It may be worth noting that the Neuroticism items are more homogeneous in loadings on the Neuroticism scale (S.D. = 0·08 Form B) than are the Extraversion items in their Extraversion loadings; in the E+ scale of Form B, for instance, S.D. = 0·16 and in the E− scale S.D. = 0·24. (The original factor analysis did not include the Lie scale items.)

The next stage in the analysis was to consider each of the 57 items in each form of the test in turn, by comparing the subjects who answered 'yes' to an item with those who answered 'no'. We wished to discover, for each item, whether these two subgroups, the 'yes' and 'no' responders, differed in their mean scores on any of the three scales of the EPI. Therefore for all 57 items in each of the two forms of the EPI, the mean Extraversion, Neuroticism and Lie scores of the 'yes' and 'no' responders were compared and any differences tested for significance using Student's t, with $\alpha = 0·05$. In making this comparison the item in question was disregarded in calculating scores on the total scales. Thus, for example, question 1 in Form A was answered 'yes' by 71·5 per cent of the subjects, and 'no' by 28·5 per cent. The Extraversion, Neuroticism and Lie means of the people who say 'yes' to this item are 13·1, 12·1 and 2·0 respectively, while the means of those who respond 'no' are 10·4, 9·4 and 2·2 respectively. Question 1 is an E+ item, i.e. saying 'yes' to it contributes a score of 1 to Extraversion. It was therefore necessary to subtract a score of one from the Extraversion mean of the 'yes' responders before testing the significance of the difference between 'yes' and 'no' responders

Table 3. *Characteristics of the Neuroticism plus items*

(Column 1 shows on which of the scales of the EPI the 'yes' responders to an item differ significantly from the 'no' responders to the item, and column 2 shows the number of items giving that type of difference. Column 3 shows the average percentage of 'yes' responders to those items. Columns 4–5 show the average factor loadings for the group of items, and columns 6–8 show for items with significant differences on any of the scales the mean size of the significant scale differences.)

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Mean factor loadings		Mean significant scale differences		
Significant scales	Number of items	Mean % 'yes'	F_1(N)	F_2(E)	E	N	L
			Form A				
None	1	8·0	0·31	0·02	—	—	—
N+	15	50·2	0·41	0·04	—	2·9	—
E− N+	4	30·4	0·48	−0·06	−1·8	3·7	—
N+ L−	3	51·4	0·30	0·09	—	2·4	−0·8
E− N+ L−	1	74·5	0·43	0·01	−2·0	3·1	−0·6
			Form B				
N+	13	40·6	0·40	0·09	—	3·2	—
E+ N+	3	54·8	0·38	0·26	1·1	2·6	—
E− N+	4	60·3	0·46	0·01	−1·5	2·8	—
N+ L−	1	55·6	0·39	0·13	—	2·6	−0·7
E− N+ L−	3	57·0	0·41	0·08	−1·2	2·6	−0·6

on Extraversion (thus the difference which was tested was between 12·1 and 10·4, not 13·1 and 10·4). For this item the people who said 'yes' differed significantly from those who said 'no' on both Extraversion and Neuroticism, with $P < 0{\cdot}05$.

Table 3 shows our analysis of the items allocated by Eysenck & Eysenck (1964) to the Neuroticism scale, all of which, it should be remembered, contribute to the Neuroticism scale score when answered 'yes'. Table 3 shows on which, if any, of the three scales, Extraversion, Neuroticism or Lying, the 'yes' and 'no' responders differed significantly.

So far, we have denoted the Eysenck scoring of an item by, for example, E+ meaning that a 'yes' answer to the item adds 1 to the total E score and E− meaning that a 'no' answer adds 1 to the total E score. Clearly, if the designation E+ is 'correct', the 'yes' subjects would have a higher Extraversion score than the 'no' subjects in that item, and conversely, if the designation E− is 'correct' the 'yes' subjects would have a lower Extraversion score than the 'no' subjects. Therefore, in considering the item analysis, an item will be said to show an E+ difference if the 'yes' subjects are significantly higher on E than the 'no' subjects, and E− difference if the 'yes' subjects are significantly lower on E than the 'no' subjects. Similar notation is used for the N and L scales.

The scale or scales on which the significant difference occurs is shown by the letters E, N or L, with a plus sign indicating that the 'yes' responders have a higher mean on that scale than the 'no' responders, and a minus sign indicating that the 'yes' responders have a significantly lower mean on that scale. When two or three letters occur together it indicates that the subjects responding 'yes' to those items differed significantly from the subjects responding 'no' on more than one of the scales. The other columns show, in turn, the numbers of items having a particular

Table 4. *Characteristics of the Extraversion plus items*

(See Table 3 for details of the entries.)

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Mean factor loadings		Mean significant scale differences		
Significant scales	Number of items	Mean % 'yes'	F_1(N)	F_2(E)	E	N	L
			Form A				
E+	3	25·8	0·11	0·40	2·8	—	—
E+ N+	1	71·5	0·24	0·46	1·7	2·7	—
E+ N−	4	50·8	−0·24	0·34	2·5	−2·3	—
E+ L+	1	10·5	0·09	0·20	2·9	—	0·8
E+ L−	2	68·8	0·11	0·51	3·5	—	−0·7
E+ N+ L−	1	40·0	0·31	0·29	2·2	1·6	−0·4
E+ N− L+	1	26·5	−0·12	0·48	3·1	−2·1	0·5
E+ N− L−	1	71·5	−0·23	0 60	3·6	−2·2	−0·5
L−	1	57·0	0·14	0·30	—	—	−0·8
			Form B				
None	2	71·8	−0·05	0·28	—	—	—
E+	7	70·6	−0·10	0·39	2·6	—	—
E+ N−	5	48·4	−0·14	0·29	1·6	−1·6	—
E+ N+ L−	1	80·2	0·17	0·21	1·2	1·4	−0·7

pattern of significant differences, the mean percentage of 'yes' responders for a type of item, the mean factor loading obtained by Eysenck *et al.* for this set of items, and in the final three columns, the size and direction of the significant differences on the three scales.

For instance in Form A, four items show an E − N + pattern of significant differences. That is, these items gave significant differences on the E and the N scales. On average 30·4 per cent of the subjects responded 'yes' to these four items. The mean of the Neuroticism factor loadings was 0·48 and the Extraversion factor loading was −0·06. The Extraversion difference was −1·8 showing that the subjects who said 'yes' to these items were on average 1·8 points lower on Extraversion than the subjects who said 'no' to these items, while the Neuroticism difference of 3·7 indicates that the subjects who said 'yes' to the items were, on average, 3·7 points higher on Neuroticism than the subjects who said 'no'. The subjects who say 'yes' to these items rather than 'no' therefore differ more on Neuroticism than they do on Extraversion, although they are significantly different on Extraversion. The items are not differentially associated with the Lie scale scores.

Reassuringly, only one of the 48 N + items in the combined EPI failed to show a significant N + difference, and 28 items show only an N + difference. The remaining 19 items have an N + difference, together with E +, E − or L − differences. Although additional differences exist, they are all smaller than the differences on the N + scale, showing that although the items are significantly related to other scale scores the relationship to their own scale is higher. These relationships are reflected to some extent in the mean factor loadings (columns 4 and 5) where the Extraversion loadings of the items are close to zero, with the exception of the three E + N + difference items from Form B, which have an average loading of 0·26.

Table 5. *Characteristics of the Extraversion minus items*

(See Table 3 for details of the entries.)

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Mean factor loadings		Mean significant scale differences		
Significant scales	Number of items	Mean % 'yes'	F_1(N)	F_2(E)	E	N	L
			Form A				
None	1	32·0	0·10	−0·26	—	—	—
E−	4	42·6	0·02	−0·18	−2·3	—	—
E− N+	3	34·2	0·22	−0·43	−3·2	2·4	—
N− L+	1	54·5	−0·19	−0·10	—	−1·3	0·5
			Form B				
E−	7	47·0	0·10	−0·22	−1·3	—	—
E− L+	1	38·1	0·29	−0·50	−3·1	—	0·4
L+	1	60·7	−0·04	−0·06	—	—	0·4

The positive significant scale differences in the Neuroticism column and the negative differences in the Lie column show that neuroticism and lying are negatively correlated in these items. Extraversion and neuroticism are negatively correlated in 12 items (E− N+) and positively correlated in three items (E+ N+). These last three items were those mentioned in the previous sentence as having large loadings on extraversion.

Table 4 shows our analysis of the Eysenck E+ items, and is similar in structure to Table 3. Three E+ items, one from Form A and two from Form B, do not have an E+ significant difference. A most notable feature of the items in Table 4 is the striking difference in results between Forms A and B, with the Form A items showing many more significant differences on the N and L scales, and showing higher mean differences on the E scale. In the combined EPI, then, E and N are positively correlated in three of the E+ items, and negatively correlated in 11 of the E+ items. The latter result gives 11 items that could be considered as N− items, thus helping to remove any response bias or stereotypy effects on the N scale. E and L are positively correlated in two of the E+ items and negatively in four items. N and L are positively correlated in one E+ item and negatively correlated in three items. Again, the factor loadings largely support the findings of the item analysis.

Table 5 shows our analysis of the Eysenck E−items, and is similar in structure to Table 3. Fifteen of the 18 E− items in the combined EPI show E− significant differences, the remaining three comprising one N− L+ item, one L+, and one item having no significant differences. The factor loadings show two remarkable E− items, namely the N− L+ item with a larger (negative) N loading than E loading, and the L+ item with negligible loadings on both N and E. E and N are negatively correlated in three of the E− items, as are E and L in one item and N and L in a further item.

Table 6 shows the analysis of the L+ items. The structure of Table 6 resembles that of Table 3, but there are no factor loadings. Four of the six L+ items in the combined EPI show significant L+ differences, the remaining two items having

Table 6. *Characteristics of the Lie plus items*

(See Table 3 for details of the entries. Columns 4 and 5 are missing, since factor loadings for the Lie scale do not exist.)

(1)	(2)	(3)	(6)	(7)	(8)
			Mean significant scale differences		
Significant scales	Number of items	Mean % 'yes'	E	N	L
		Form A			
None	1	21·0	—	—	—
L+	1	8·5	—	—	0·7
E− L+	1	51·0	−1·2	—	0·4
		Form B			
None	1	30·2	—	—	—
L+	1	18·7	—	—	0·5
N− L+	1	20·6	—	−1·4	0·6

Table 7. *Characteristics of the Lie minus items*

(See Table 6 for details of the entries.)

(1)	(2)	(3)	(6)	(7)	(8)
			Mean significant scale differences		
Significant scales	Number of items	Mean % 'yes'	E	N	L
		Form A			
None	1	75·5	—	—	—
L−	1	83·5	—	—	0·5
E+ L−	1	62·0	1·5	—	−0·4
N+	3	85·7	—	2·1	—
		Form B			
None	1	88·1	—	—	—
L−	1	91·7	—	—	−1·2
E+ L−	2	84·3	2·8	—	−2·1
N+ L−	1	89·3	—	2·0	−0·9
E+	1	90·9	3·6	—	—

no significant scale differences. All except the E− L+ item are answered 'yes' by a minority of subjects.

Table 7 shows the analysis of the L− items. The structure resembles that of Table 6. Six of the 12 L− items in the combined EPI show significant L− differences. The remainder comprise two items with no significant scale difference, three N+ items and one E+ item. All these items are answered 'no' by a minority of subjects.

Summarizing these data, then, Form A has 31 significant N+ items and Form B 26. There are seven and six significant N− items in Forms A and B respectively. The E+ significant items total 15 in Form A and 19 in Form B, while there are 13 Form A and 15 Form B E− significant items. Considering Lie scale differences, there are five Form A and four Form B L+ significant items and 11 Form A and nine Form B L− significant items. Finally, four items from each form of the test have no significant scale differences. Table 8 shows the items and the scales on

Table 8. *The scales to which the items significantly contribute*

(The asterisk shows items having greater than average significant difference.)

E+ Form		E− Form		N+ Form		N− Form		L+ Form		L− Form		None Form	
A	B	A	B	A	B	A	B	A	B	A	B	A	B
1	1	5	5*	1	2*	3*	3	6	5	8	7	32	8
3	3	6	9	2	4*	25	13	24*	6	11	9	36	36
8	10*	11	14	4	7	27	22	34	24	13	12	43	54
10*	13	15*	15	7*	9	34	24	44*	34	17	14	48	56
13*	17	20	19	8	11*	39	25	53		19	30		
17*	18	26	20	9*	12	49*	39			22	31		
25*	22	29*	29	11	14	53				25	42*		
27*	25	35*	31	12	16*					28	48*		
39*	27	37*	32	14*	19					42	49		
44*	28	41	37	15	21*					45			
46	30	47	41	16*	23					54			
49	33	51*	50*	18	26								
53*	39	52	51*	19	28								
54	44		55	20	31								
56*	46		57	21	33								
	48*			23	35								
	49			26*	38*								
	52			28*	40								
	53*			30	43*								
				31	45								
				33	47								
				35*	49								
				38	50								
				40*	52								
				45	55								
				47*	57								
				50*									
				51									
				52									
				55									
				57									

which they have significant differences, with items having more than one significant difference appearing more than once.

If one simply wished to construct a test on the basis of these items there is a large pool of items, some of which would contribute to only one scale, others to more than one scale. Various possibilities exist for balancing the number of 'yes' and 'no' items, or for picking items which contribute to one scale only.

A shorter test could be constructed on the basis of the best items in the two forms of the EPI. In this case only those items with greater than average differences between 'yes' and 'no' responses could be considered. They are marked with an asterisk in Table 8.

If one wished to produce a very short test which would detect extremely introverted or extraverted, stable or neurotic, lying or non-lying people, then the items which are related to very high or low scores are required. They are shown in Table 9. A test of this kind would be of no use in quantifying a person's degree of possession of the trait: it would have been designed to pick out extreme types.

Table 9. *Items associated with extreme subgroups on the three scales, Extraversion, Neuroticism and Lying*

(Column 1 gives the number of the item producing the extreme subgroup, Column 2 indicates the scale on which the subjects are extreme, Column 3 shows whether the extreme groups responded 'yes' or 'no' to that item, Column 4 shows the mean score on the appropriate scale of the extreme subgroup, Column 5 gives the z score for the scores in Column 4, Columns 6–8 show how the Eysencks scored the item, what significant scale differences were found in the earlier part of this study, and the percentage of people responding 'yes' to that item.)

(1) Item number	(2) Extreme scale	(3) 'Yes' or 'no'	(4) Subgroup mean	(5) z	(6) Eysenck scale	(7) Significant scales	(8) % 'yes'
				Form A			
44*	E	Yes	14·9	0·60	E+	E+ L+	10·5
17*	E	No	8·7	−0·88	E+	E+ L−	77·0
47*	N	Yes	14·1	0·63	N+	E− N+	28·5
7*	N	No	6·9	−1·05	N+	N+	77·5
19	L	No	2·9	0·64	N+	N+ L−	80·5
6	L	No	1·3	−0·50	L−	E− L+	51·0
				Form B			
13	E	Yes	16·7	0·62	E+	E+ N−	36·1
10*	E	No	9·3	−1·38	E+	E−	91·3
38*	N	Yes	14·9	0·77	N+	N+	14·3
2*	N	No	7·6	−0·93	N+	N+	69·4
48*	L	No	4·7	2·54	L−	E+ L−	98·4
30	L	Yes	0·9	−0·39	L−	E+ L−	70·2

The data in Table 9 show that the nature of the scale difference produced is an important property of an item, in that it may produce a subgroup of subjects who are extremely high or extremely low on a scale. These extreme subgroups are usually a small minority of the subjects, and only one of the items in Table 9 is close to having a 50 per cent 'yes' response rate. The 'no' responders to this item (item 6 in Form A) constitute a low lie score group. Items which produce extreme subgroups would in certain situations be especially valuable in a personality questionnaire, and could provide the basis for shorter tests designed for specific purposes, such as the detection of neurotics in clinical work, the selection of extremely extraverted and introverted groups for experimental work, or the identification of very stable individuals for occupational purposes. The matter of item detection and personality stimulation with such tests would, however, require investigation.

Examination of these extreme subgroup items reveals that the Extraversion items are concerned with sociability and the Neuroticism items with mood swings, self-confidence and the ability to put thoughts into words quickly. The Lie extreme subgroup items are as would be expected, except one N + item, item 19 of Form A, which also shows a significant N + difference.

This study, then, shows how the EPI items behave with a sample of normal undergraduates. Clearly, the original scoring can be improved on, using Table 8 to produce a modified scoring key based on this particular sample of subjects. The extent to which these results can be generalized would, however, need to be tested empirically. The question of the use of the EPI in clinical work may be partly answered by the extreme subgroup analysis on non-psychiatric groups, but the ultimate test must lie in the study of data from patients. Additional refinements are possible, in that items may be given different score values, items could be added or discarded to give equal numbers of + and − items for each scale, helping to overcome difficulties caused by response bias and stereotypy.

In the introduction, we stated that the aim of the analyses carried out here was to purify the test and to examine the properties of the items. It would be highly undesirable, however, if our efforts produced a more homogeneous test (Cattell & Tsujioka, 1964) or a test in which the factors are more tautological (White *et al.*, 1969). The latter authors state that personality factors lie on a continuum from tautological factors to combinatorial factors. An extreme tautological factor would consist of several repetitions of the same question with minimal changes in wording. An item analysis would merely produce an indication of the reliability of the items. The Extraversion and Neuroticism scales of the EPI are towards the other end of the continuum, having many dissimilar items measuring several differing, if related, factors such as sociability, impulsiveness and mood swings (White *et al.*, 1969). An individual item would not be related to the total scale score if it did not correlate with the other items comprising that scale. A personality scale with many such items would be factorially impossible, as well as being extremely difficult to label.

Very few of the EPI items, however, have been shown here to be completely redundant. What we have shown is that the majority of the items in each of the Eysenck scales do correlate with the scale total that the official scoring intends, but in addition, the items may correlate with other scale totals. Some items appear to be more strongly related to a scale other than that which was originally intended, and a new type of item, the N−, has been shown to exist in the EPI.

We have therefore clearly not made the EPI more tidy at the expense of inflicting undesirable homogeneity on it. On the contrary, fuller use of the factorial complexity of many of the items can now be made.

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References

Cattell, R. B. & Tsujioka, B. (1964). The importance of factor-trueness and validity, versus homogeneity and orthogonality, in test scales. *Educ. psychol. Measur.* **24**, 3–30.

Eysenck, H. J. & Eysenck, S. G. B. (1964). *Manual of the Eysenck Personality Inventory.* London: University of London Press.

Eysenck, H. J., Hendrickson, A. & Eysenck, S. G. B. (1969). The orthogonality of personality structure. In H. J. Eysenck & S. G. B. Eysenck (eds.), *Personality Structure and Measurement.* London: Routledge & Kegan Paul.

Huggan, R. E. (1968). Neuroticism, distortion and objective manifestations of anxiety in males with malignant disease. *Br. J. soc. clin. Psychol.* **7**, 280–286.

Kissen, D. M. (1966). Psychosocial factors, personality and prevention of lung cancer. *Med. Offr* **116**, 3032, 135–138.

Power, R. P. (1975). Liars, damned liars, and malingerers: the faking of extraversion, introversion and neuroticism. *Irish J. Psychol.* **3** (in the Press).

Power, R. P. & MacRae, K. D. (1971). Detectability of items in the Eysenck Personality Inventory. *Br. J. Psychol.* **62**, 395–401.

Power, R. P., Muntz, H. J. & MacRae, K. D. (1975). Man or machine as diagnostic tool: a comparison between clinical psychologists and discriminant function analysis. *Br. J. soc. clin. Psychol.* (in the Press).

White, P. O., Soueif, M. I. & Eysenck, H. J. (1969). Factors in the Eysenck Personality Inventory. In H. J. Eysenck & S. B. G. Eysenck (eds.), *Personality Structure and Measurement*. London: Routledge & Kegan Paul.

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PERSONALITY AND DIFFERENTIAL SUSCEPTIBILITY TO HYPNOSIS: FURTHER REPLICATION AND SEX DIFFERENCES

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Following the study of Gibson & Curran (1974), a further sample of 45 subjects was tested on the Eysenck Personality Inventory (EPI) and a slightly modified form of the Stanford Hypnotic Susceptibility Scale (SHSS) in precisely the same way. The results in this second sample were broadly the same as those obtained in the earlier study. Combining the two samples, it was found that the sex variable provided some interesting contrasts. The power of the lie scale of the EPI to predict hypnotic susceptibility observed earlier was found to be a significant effect only for males. While there was no significant difference between the sexes in terms of the means and S.D.s of the extraversion (E) and neuroticism (N) scales, when the interaction of these scales was studied males and females differed significantly. The population from the two studies ($n = 88$) was analysed by means of polar coordinates in the manner suggested by Eysenck (1966) with regard to the E and N scales. Eysenck's prediction as to hypnotic susceptibility was strikingly confirmed. These data are briefly discussed in terms of alternative approaches to hypnosis from the 'state' and the 'non-state' viewpoints.

A recent study (Gibson & Curran, 1974) was designed to replicate work on the personality correlates of differential hypnotic susceptibility which had been done at the Maudsley (Furneaux & Gibson, 1961) and at Stanford (Hilgard & Bentler, 1963). In brief, the Maudsley study, using the Maudsley Personality Inventory (MPI), had shown that subjects who scored high on the lie scale tended to be insusceptible to hypnosis; moreover, 'stable extraverts' (SE) and 'neurotic introverts' (NI) tended to be the most susceptible, and 'neurotic extraverts' (NE) and 'stable introverts' (SI) the least susceptible. The Stanford study produced results which were not only statistically significant but were diametrically opposed in direction.

The 1974 study of Gibson & Curran used the more modern personality test, the Eysenck Personality Inventory (EPI), and more sophisticated means of measuring hypnotic susceptibility (Curran & Gibson, 1974). The results supported the earlier Maudsley study but were based on a rather limited number of subjects ($n = 43$). The present study reports further work replicating the 1974 study exactly, with an independent sample of 45 subjects. It is reasoned that only by testing independent samples will the controversy eventually be resolved and some insight be gained into the extremely puzzling interaction between personality variables which account for differential susceptibility to hypnosis. This work is important not only in the field of hypnotic research, but because of its implications for the whole methodology of assessing personality by means of written questionnaires.

METHOD

Subjects

The subjects were 45 students comprising the majority of two later years' intake on the degree course in psychology which supplied the subjects for the earlier 1974 study. All were volunteers, and no pressure to participate was used, in contrast to the Stanford study.

Table 1. *Basic data of samples* 1 *and* 2 *compared*

	Sample 1		Sample 2	
	Mean	S.D.	Mean	S.D.
SHSS	4·91	3·23	4·82	3·40
Lie	2·91	2·03	3·27	2·50
Extraversion	26·60	9·57	25·93	7·13
Neuroticism	23·98	8·15	23 80	9·31

Materials and procedure

These were identical with the earlier study. A full account of the procedural details is now published elsewhere (Curran & Gibson, 1974) with the factorial structure of the Stanford Hypnotic Susceptibility Scale (SHSS) as found.

RESULTS

Between sample comparisons

The basic data of the two samples in terms of the means and S.D.s of the four scales used are shown in Table 1. None of the differences between the two samples is statistically significant; in fact they are closely similar on all scales.

Lie scale and hypnotic susceptibility

In sample 1, as reported by Gibson & Curran (1974), both the L scale and the SHSS were dichotomized at their medians and shown to be negatively related. The relationship is shown below and compared with the same treatment applied to sample 2, the SHSS being dichotomized at $\leqslant 4$.

Sample 1

		L −	L +
SHSS	+	13	9
	−	7	14

$r_t = -0{\cdot}40$, n.s.

Sample 2

		L −	L +
SHSS	+	13	6
	−	11	15

$r_t = -0{\cdot}40$, n.s.

The relationship between the two scales in terms of r_t is precisely the same for these two independent samples dichotomized at their respective medians. In fact, as the L scores for sample 2 were slightly higher than for sample 1 (as shown in Table 1) the median was 1 point higher.

Extraversion, neuroticism and susceptibility

In the 1974 study following the method employed at Stanford by Hilgard & Bentler (1963), the cases in the SE and NI quadrants were pooled and compared with the pooled NE and SI quadrants with respect to hypnotic susceptibility. This treatment was also applied to the data of sample 2 and the results are compared below:

Sample 1

	SHSS −	SHSS +
SE and NI	9	17
NE and SI	12	5

$r_t = 0{\cdot}54$, $\chi^2 = 3{\cdot}98$, $P < 0{\cdot}05$

Sample 2

	SHSS −	SHSS +
SE and NI	9	10
NE and SI	17	9

$r_t = 0{\cdot}26$, n.s.

Table 2. *Basic data of the two sexes compared*

	Males		Females	
	Mean	S.D.	Mean	S.D.
SHSS	3·89	2·63	5·62	3·08
Lie	3·45	2·55	2·88	2·06
Extraversion	25·03	7·71	27·18	8·82
Neuroticism	22·11	9·51	25 06	7·60

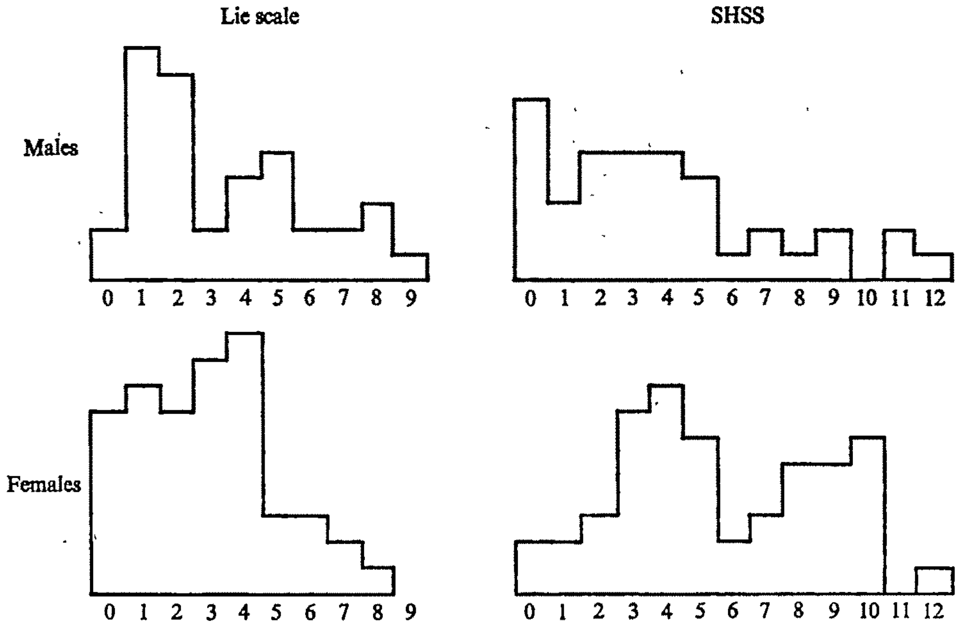

Fig. 1. Histograms of score distributions of males and females on the Lie scale and SHSS.

As shown above, the interaction in sample 2 is the same as in sample 1, although it does not reach statistical significance. In further analyses of the data in order to examine any possible differences between the two samples which might account for a lowered interaction, it was found that they differed with regard to their proportional sex composition.

Between sexes comparisons

The sex composition of the two samples was as follows:

	Sample 1	Sample 2
Males	16	22
Females	27	33

The basic data of the two sexes in terms of the means and S.D.s of all the scales are shown in Table 2.

Lie scale, hypnotic susceptibility and sex

From Table 2 it might be concluded that there was little difference between the sexes on the L scale or the SHSS. A very different picture is presented if we study

Table 3. *Interaction between extraversion and neuroticism with respect to sex, with mean L score in parenthesis*

	Males	Females	Total
Stable extraverts	9 (2·44)	15 (2·73)	24
Neurotic extraverts	9 (2·89)	13 (2·77)	22
Stable introverts	15 (4·73)	5 (3·80)	20
Neurotic introverts	5 (2·40)	17 (2·82)	22
Total	38	50	88

$\chi^2 = 12{\cdot}35$ with 3 d.f.; $P < 0{\cdot}01$.

the actual histograms of scores as shown in Fig. 1. Whereas the mean L score of males is shown to be slightly higher than that of females, if their modal L scores are compared, the male modal score is 1 and the female one is 4. The male L distribution tends to be considerably more skewed and a median cut of 19/19 occurs at $\leqslant 2$. Comparing the male L scores with their SHSS scores (as was done earlier in this analysis for the two samples dichotomizing the SHSS at $\leqslant 5$), we get the following:

		L	
		−	+
SHSS	+	11	2
	−	8	17

$r_t = -0{\cdot}76$, $\chi^2 = 7{\cdot}48$, $P < 0{\cdot}01$

Dichotomizing the female L scores at the same median cuts on both scales, we get the following:

		L	
		−	+
SHSS	+	12	16
	−	10	12

$r_t = 0{\cdot}04$, n.s.

Thus in contrast to the significant power of the L scale to predict hypnotic susceptibility among the males, the L scale has no such power among the females. Several exploratory cutting points were also tried in the female distributions (including their own group median) but no relationship could be found.

Comparisons with extraversion and neuroticism

The E and N scores shown in Table 2 imply that there is no difference between the sexes on these two scales. However, if we examine the *interaction* between the two scales as in Table 3, a significant difference is apparent. The whole population has been divided as equally as possible into four quadrants and among the more extraverted half of the population sex has little bearing on the distribution. Among the more introverted half, however, sex makes a very significant difference; male introverts are predominantly on the stable side and female introverts on the neurotic side.

The mean L scores of each quadrant are shown in parenthesis and it can be seen that for both males and females the highest mean L score is in the SI quadrant. The statistical significance of this was tested by dichotomizing the L scale as before, and on a 2×2 table comparing the L scores in the SI quadrant with those in all other quadrants pooled. For the males the difference is significant ($\chi^2 = 3{\cdot}96$; $P < 0{\cdot}05$).

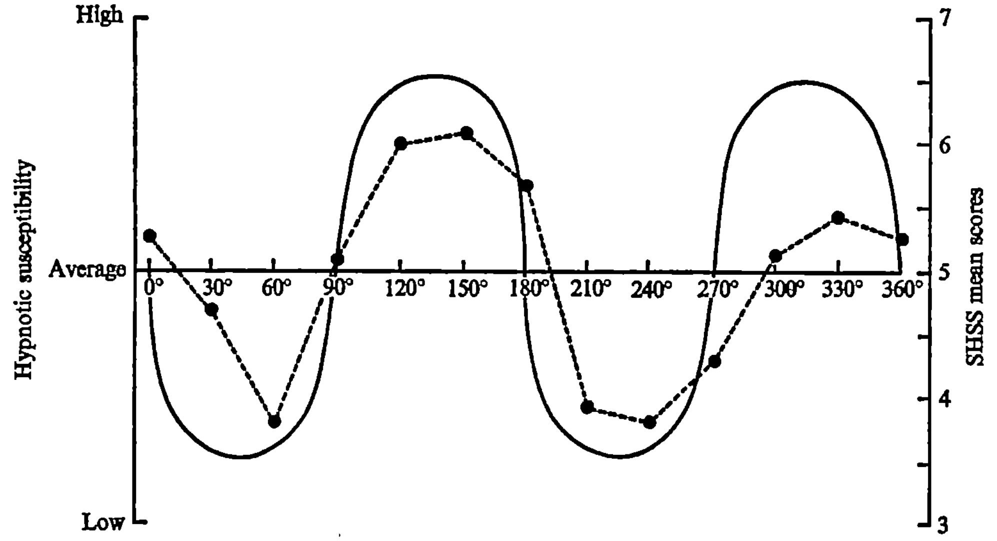

Fig. 2. Polar coordinates of the SHSS related to E and N, superimposed on the curve predicted by Eysenck (1966).

The interaction between E, N and SHSS is shown below for males and females separately.

	Males		Females	
	− SHSS	+	− SHSS	+
SE and NI	7	7	12	20
NE and SI	18	6	10	8

$r_t = 0{\cdot}41$, n.s. $\qquad r_t = 0{\cdot}28$, n.s.

The interaction of the two EPI scales with hypnotic susceptibility is thus seen to be in the same direction for both sexes, although the difference between the sexes with regard to the interaction of E with N has been shown to be significant in Table 3. If both sexes are considered in one population of 88 subjects, we get the following interaction:

	−	SHSS +
SE and NI	19	27
NE and SI	28	14

$r_t = 0{\cdot}39$, $\chi^2 = 4{\cdot}70$, $P < 0{\cdot}05$

Analysis by polar coordinates

Analysis by means of dichotomizing scales invariably leads to considerable error variance in the outcome. An alternative form of analysis has been suggested by Eysenck (1966) with regard to relating E and N scores to hypnotic susceptibility. Eysenck suggested the use of polar coordinates and his actual figure has been reproduced in Fig. 2 (Fig. 8, Eysenck, 1966). Superimposed on Eysenck's theoretical curve, which he predicted would represent the relationship between suggestibility scores and the polar coordinates of E and N, are the actual data found in this study. These points were arrived at by dividing the scatter-plot on the E and N axes into 12 30° segments. Each point represents the mean of the SHSS scores lying 30° ± each of the radial cutting lines. By this method of determining the curve each individual contributes to two means and a smoother curve is obtained. It will be observed that the curve thus empirically determined conforms extremely well to the wave form which Eysenck predicted.

DISCUSSION

This study set out to do the limited task of replicating the study of Gibson & Curran (1974) with a second sample of subjects. As a total population of 88 subjects was thereby tested, certain interesting interactions with sex were discovered by serendipity, and hence a broader discussion of the significance of these results seems warranted.

Most of the major theorists in the field of hypnotic research take one of two alternative approaches. This difference is sometimes referred to in terms of the 'state' and 'non-state' viewpoints of hypnosis (Coe, 1973). The first approach treats hypnosis as a presumed altered state of awareness or 'trance' (e.g. Hilgard, 1965) as it is also considered in the traditional Pavlovian work (e.g. Pavlov, 1923). The second approach does not consider such an assumption necessary and tends to view hypnosis more in terms of the subject's goal-directed strivings and role-playing skills (e.g. Sarbin & Coe, 1972). All theorists are agreed, however, that there exists a real and reliable inter-subject difference in susceptibility. The former approach can be extended to suggesting that there are reliable personality differences, perhaps having a physiological basis, underlying differential susceptibility. The latter approach would lead us to place more importance on situational variables and explain differential susceptibility more in terms of the 'will' of the subject.

The work that has been reported here can be seen from either approach. No very strong association between personality parameters as measured by the EPI, and hypnotic susceptibility as measured by the SHSS, has been demonstrated, but at least the type of association has been shown to be *reliable* by the similar results coming from three independent studies. The unexpected power of the L scale to predict susceptibility in male subjects tends to support the argument that susceptibility is *situational* rather than a function of enduring personality parameters. Work on L scales shows that they may measure situational variables; subjects will 'fake good' or 'fake bad' according to the circumstances in which the test is administered (e.g. Salas, 1968). However, the whole of the association cannot be explained as an artifact of the L scale, for it is manifest with females also, and the L scale does not predict hypnotic susceptibility for them.

It is pertinent at this juncture to discuss why the L scale should be different both in distribution (see Fig. 1) and implication for the two sexes. Males are generally less well-behaved than females and society recognizes this difference, e.g. in the grossly different rates of delinquency among males and females. This difference is certainly reflected in children's lie scale data where males score fairly consistently lower than females (Gibson, 1964; S. B. G. Eysenck *et al.*, 1966). One might have expected the male students in this study to have a lower L scale distribution than the female students; instead, the males have a lower mode but a higher mean due to the more pronounced skew of their distribution. It is suggested that those males in the upper part of the distribution are exhibiting a tendency to resist experimental investigation of their personality parameters, and that this defensive trait is manifest in the hypnosis situation. Higher L scores for females may be closer to objective self-report and thus have less implication of defensiveness which might imply resistance to hypnotic induction. The 'high liars' have not been excluded from the analysis as in the Maudsley study, and it may be seen that because they are rather more frequent in the NI quadrant, this enhances the observed effects, at least for males. It is no more meaningful to state that 'high liars tend to represent themselves as stable introverts' than it is to state that 'stable introverts tend to give high lie scores'. One cannot determine which is the more meaningful statement.

In the manual of the EPI it is stated that 'Correlations with sex are not large, because items giving large sex differences were eliminated during the construction of the inventory' (Eysenck & Eysenck, 1964, p. 18). This may be so, but the present study has shown a significant difference between the sexes with regard to the *interaction* of E and N. It will be of interest to see if other researchers using the EPI find the same sex difference if they use the same method of interaction analysis rather than just reporting means and S.D.S.

If the data on the L scale in relation to hypnotic susceptibility, for males, lends some support to the situational, 'non-state' view of hypnosis, the study as a whole cannot be said to do so. It must be acknowledged that Eysenck (1966) has had his prediction strikingly confirmed by the data shown in Fig. 2. All the detailed qualifications of L scale differences, of sex differences and various interactions do not override the lawful relations between E and N with regard to hypnotic susceptibility which are apparent when we study a large enough population. If Eysenck's theory is sound (Eysenck, 1967) then there is an underlying biological basis for the parameters of E and N and this would be in harmony with a 'state' theory of hypnosis.

It must be admitted regretfully that these additional data bring us no nearer the resolution of the conflict between the Maudsley and the Stanford studies referred to earlier. Re-reading the latter study we find that 87 per cent of their subjects were males, a feature which makes their finding of a *positive* association between high L scoring and hypnotic susceptibility even more surprising in the light of the findings of the present study, and which calls for independent testing in other laboratories.

REFERENCES

COE, W. C. (1973). Experimental designs and the state–nonstate issue in hypnosis. *Am. J. clin. Hypnosis* **16**, 118–128.

CURRAN, J. D. & GIBSON, H. B. (1974). Critique of the Stanford Hypnotic Susceptibility Scale: British usage and factorial structure. *Percept. mot. Skills* **39**, 695–704.

EYSENCK, H. J. (1966). Personality and experimental psychology. *Bull. Br. psychol. Soc.* **19**, no. 62, 1–28.

EYSENCK, H. J. (1967). *The Biological Basis of Behaviour*. Springfield, Ill.: Thomas.

EYSENCK, H. J. & EYSENCK, S. B. G. (1964). *Manual of the Eysenck Personality Inventory*. London: University of London Press.

EYSENCK, S. B. G., SYED, I. A. & EYSENCK, H. J. (1966). Desirability response set in children. *Br. J. educ. Psychol.* **36**, 87–90.

FURNEAUX, W. D. & GIBSON, H. B. (1961). The Maudsley Personality Inventory as a predictor of susceptibility to hypnosis. *Int. J. clin. exp. Hypnosis* **9**, 167–177.

GIBSON, H. B. (1964). A lie scale for the Junior Maudsley Personality Inventory. *Br. J. educ. Psychol.* **34**, 120–124.

GIBSON, H. B. & CURRAN, J. D. (1974). Hypnotic susceptibility and personality: a replication study. *Br. J. Psychol.* **65**, 283–291.

Br. J. Psychol. (1975), 66 4, *pp.* 521–541
Printed in Great Britain

BOOK REVIEWS

Race Differences in Intelligence. By JOHN C. LOEHLIN, GARDNER LINDZEY and J. N. SPUHLER. San Fransisco: Freeman. 1975. Pp. xiv+380. $12.00; paper, $5.95.

Loehlin, Lindzey and Spuhler, two psychologists and a human geneticist, are greatly to be commended for discussing the controversial topic of racial differences in intelligence with dispassionate competence. Their book was prepared under the auspices of the American Social Science Research Council with some financial support from the U.S. Office of Child Development.

The discussion is limited to differences in intelligence test scores between sub-populations in the United States, and largely to differences between Whites, i.e. those of European origin, and Blacks, i.e. those whose origin is in most part from West or Central Africa; though there is some consideration also of Orientals (Chinese and Japanese) and Amerindians. The basic question under review is whether the observed differences in intelligence scores between these 'races' are due to genetic differences or environmental differences or a mixture of both.

In part I of the book the authors consider the biological concept of race, the validity of measurements of intelligence, and the concept of heritability. They record some of the evidence that the major living races of man may reasonably be regarded as sub-species. The differences are certainly not at the full specific level. All races of man are fully interfertile (and have essentially the same complement of chromosomes). There is archaeological evidence that the main sub-species were differentiated at least 30,000 years ago and perhaps much earlier. Calculation of genetic 'distance' from blood groups and plasma proteins suggests that Negroids, Mongoloids and Caucasoids have been separated for 20,000–40,000 years, with the longest separation between Mongoloids and Negroids, and the shortest between Mongoloids and Caucasoids. The main dispersal could have been appreciably earlier, with the distance betweeen the main races reduced by a certain amount of intermigration.

On the issue of whether intelligence is best viewed as single general capacity, or a series of special abilities that tend to be positively intercorrelated, the authors comment that most of the time it does not matter. On heritability and the analysis of variance of intelligence test score the authors explain clearly the main concept and discuss the possible contributions of covariance, interaction, epistasis and assortative marriage. The authors make the important point that an estimate of heritability of a character refers to a particular trait in a particular population at a particular time. They discuss the limitations of measurements of heritability and note that such measurements are available at present mainly for Caucasian populations in the United States and Europe. They point out that estimates of broad heritability mostly lie between 0·60 and 0·85, and in an appendix dispose effectively of a recent claim that the data are consistent with zero heritability. The authors stress that high heritability of a trait within a population does not necessarily imply that no environmental training can modify that trait, but does imply that minor changes in environmental factors that already vary widely in a population are not likely to produce large changes in the levels of the trait. They also rightly stress that estimates of heritability within a population provide little, if any, information about the heritability of differences between the means of sub-populations. They note, however, that high within-group heritability does suggest that, if the between-group differences are largely environmental, the critical environmental factor concerned is likely to be one which sharply contrasts the groups. Such a factor would be racial prejudice. The algebra of heritability calculations is given in an appendix.

In part II the authors summarize the observed differences in mean test score between races in America – essentially a significantly higher mean in Whites and Orientals than in Blacks and Amerindians. They then discuss the small amount and equivocal nature of the evidence that is yet available on the aetiology of these group differences. There is no consistent evidence of differences in heritability within the white and black populations of the U.S. There is no evidence that high scoring Blacks tend to be those with a substantial amount of white ancestry; but the last research study of this type was carried out nearly 30 years ago. The illegitimate offspring in Germany of white and black American soldiers do not show any significant differences in mean intelligence test score, suggesting no genetic difference between these particular samples of the white and black U.S. populations. On the whole Black–White differences have shown little change over several decades either in school children or military recruits; but some sub-populations have shown marked changes over this period, demonstrating that environmental changes can substantially alter the mean level of a group. With respect to the age at which racial differences appear, most studies show that differences on test score are well marked by 3 to 4 years of age, but not present at 1 or 2 years of age. This may well be because tests before the age of 3 or 4 do not measure 'intelligence'. During the school years the Black–White differences remain in general unchanged, though the scores of both white and black rural children fall over this period in comparison with urban white and black children.

Socio-economic status (SES) is correlated with race in the United States. Those who regard SES differences in intelligence test score as largely due to environmental factors would see some of the Black–White differences as reflecting differences in SES grouping. It is, however, plausible to attribute SES differences at least in part to genetic differences, since social mobility up or down in each generation will be in part dependent on intelligence. In contrast there is no *a priori* reason to suppose that racial differences in one particular direction should be in part genetic. Further, mean test scores of Blacks are lower than those of Whites within each SES group. Black children of parents of high SES status tend to regress towards the black mean and white children towards the white mean; but the authors correctly point out that this would be expected whether the variation is due to genetic or environmental factors. Studies of racial profiles of intelligence are of interest, though again do not contribute to the main problem of the aetiology of the differences. Black children tend to score relatively well in verbal tests, less well in reasoning, and poorly in number and space tests. This is seen in both middle and lower SES groups, and is the opposite to the pattern seen in Amerindian and Oriental children. The latter often surpass Whites in numerical and spatial tests. In the last section of part II and an appendix the authors give a full account of the possible relationship of nutrition and intelligence. Here the authors appear to give too much weight to the possible effects of malnutrition on intelligence, perhaps as a result of experience limited to the United States, though they rightly note that some populations keep healthy and active (and intelligent) on food intakes which are grossly inadequate by current U.S. standards.

In part III the authors summarize the empirical findings and state their main conclusions. These are that observed racial differences are in part environmental, in part genetic and in part due to inadequacies of the tests themselves. They stress, however, that racial differences must be seen in the perspective that the major part of variation is within and not between racial groups. They also emphasize that any differences observed should not be felt as a humiliation and should not be the occasion of a denial of opportunity in education, employment or social participation. They point out, however, that if the aim is to reduce race differences in intelligence test scores it is important to know the cause. The effect of some genetic differences might perhaps be more easily modified than environmental differences; but first the specific genetic and environmental factors must be elucidated. For the design of educational policies the authors stress that research into individual differences is more important than research into near-race differences.

This book is much to be commended for technical competence and objectivity. Unfortunately, there are too few data to resolve the main issue, and indeed it is difficult to see how experiments that are presently practicable could be devised to resolve the issue in a way that would satisfy the scientists.

C. O. CARTER

Social Behaviorism. By A. W. STAATS. Homewood, Ill.: Dorsey Press. 1975. Pp. ix + 655. $13.95.

Staats has attempted over the years to put forward a general, unified theory of human behaviour. His earlier books – *Complex Human Behaviour; Human Learning; Learning, Language and Cognition; Child Learning, Intelligence, and Personality* – are now complemented by what is apparently the final work in this series. The work of Arthur Staats (sometimes complemented by that of Carolyne Staats) has always been based on a Skinnerian approach, but has deviated from the usual very narrow specialization of operant conditioners, and their refusal to take the work or contribution of 'outsiders' very seriously, by being more accessible to criticisms and to experimental findings from outside the fold. (The hagiological literature that has grown up around Skinner is second only to that which has grown up around Freud!) In this book, Staats has attempted two things. In the first place, he has attempted to formulate general principles which would unite Pavlovian and Skinnerian approaches, and in particular associate them with the known facts of emotional and motivational research. In the second place, he has attempted to extend these principles to the social field, both by attempting to explain the principles of personality and intelligence, and by applying them to such fields as behaviour therapy. These are both worthwhile attempts, and the book deserves a warm welcome, even though on both grounds there may be many criticisms possible. There is no doubt that psychology is short of unifying principles, and the way in which different 'schools' neglect and disregard each other's work is a disgrace to what purports to be a science. Any effort in the direction of such unification is therefore an important event.

Staats begins by pointing out that many of the early theorists regarded classical and operant conditioning as different principles, and that even one-process theories like Hull's soon developed into two-process learning theories. Staats insists that 'there is overlap and interaction of various sorts between classical and instrumental conditioning...Basic to consideration of the classical and instrumental conditioning overlap is the fact that most UCSs have two functions. Most stimuli that function to elicit responses in classical conditioning will also function as reinforcers... Thus, in actuality...every UCS is also an RS. It may be convenient to label only the single function with which one is concerned in any particular case. But it should be recognized that such a stimulus is actually a UC–RS – a stimulus with double functions.' This conceptualization (which has something in common with the reviewer's concept of incubation of fear responses) leads to the conclusion that the conditioned stimulus would come to elicit a response which would include *both* of the stimulus functions of the unconditioned stimulus.

Staats includes emotional states in his conditioning paradigm in the following manner. 'In any instrumental conditioning situation, the instrumental response is conditioned to the emotional response that the directive stimulus elicits. The reason this is of major importance is because the organism learns a large number of instrumental responses that are elicited by positive emotional responses, and a large number elicited by negative emotional responses.' Once the animal has learned this mechanism, 'anything which increases the tendency of a stimulus situation (DS) to elicit a positive emotional response in the organism will increase the tendency of this situation to elicit one of the...approach responses that the organism already has in its repertoire. Thus, classical conditioning procedures that condition an emotional response to a stimulus will also increase the power of that stimulus to elicit overt instrumental behaviors. In this way classical and instrumental conditioning are inextricably interrelated.' Staats goes into considerable detail, and argues his case forcibly; parts of what he has to say are of course not new, but the design of his system is well worth consideration. The first part of his attempt must be judged successful on the whole.

The second part is much more subject to criticism. Staats tries to account for the existence of trait and ability concepts in human beings in terms of learning theory, and within limits such an attempt is certainly worthwhile; undoubtedly people change and develop in accordance with environmental stimuli along predictable ways. But they are also predisposed, innately, to react in certain ways to identical stimuli; neither 'heredity' nor 'genetics' figures in the book's subject index. Nor does Staats deal with the many facts (some of which I tried to summarize in *The Biological Basis of Personality* and *The Measurement of Intelligence*), which make any attempt to leave out biological determinism an idle pastime. It is always easy to establish a point of view, and make it appear appealing, by leaving out the evidence which contradicts it; there is no proper discussion of such evidence, and consequently the book can hardly be recommended for use as a text-book – except perhaps in the U.S.A., where unthinking environmentalism is rife. Staats' discussion is philosophical rather than factual; where facts are adduced, they are highly selected. Thus we are told of an experiment in which certain types of answers to personality tests of sociability were conditioned by reinforcement, but not of the large number of twin experiments, or experiments with adopted children, demonstrating high heritability for this trait. This is hardly playing fair with the student who expects his text-books to present a fair picture of both sides of the argument.

On the whole the book might be recommended to more mature students with a good background in learning theory, personality theory, psychometrics and social psychology; they should be able to evaluate the contents and make allowance for bias. They will undoubtedly find the volume stimulating, interesting, and thought-provoking. Not many books can have this much to be said for them.

H. J. EYSENCK

Obedience to Authority. By Stanley Milgram. London: Tavistock. 1974. Pp. xvi + 224. £2.50.

It sometimes seems as if social psychologists have all the fun. Whilst others drudge away at apparent movement, classical conditioning, dichotic listening, figural after-effects, maze learning, nonsense syllables, reaction times, vocabulary tests, weight-lifting or whatever, the social psychologist apparently investigates only human beings in situations having an immediate bearing on real life. This is doubtless an illusion, but if so it is one to which Milgrams's extremely important book contributes. The question of why, and under what circumstances, people will obey orders to do things which they believe to be immoral is one which is crucial for the understanding of such contemporary phenomena as Nazi war crimes, Watergate and even the recent assertions in this country that the law *must* be obeyed (echoes of *Befehl ist Befehl*!), no matter how unjust or oppressive it may be.

As is probably well known by now, the basic situation used by Milgram is one in which the subject is instructed to act as the Teacher in a verbal learning experiment, and to administer to the Learner a series of electric shocks whose strength increases from 15 volts through to 420 volts (marked 'Danger: Severe Shock') to a level designated by 'XXX'. As the intensity goes up the Learner cries out in ever greater agony, demands to be got out, complains of a heart condition, and finally becomes completely silent. Of course, the Learner is really a confederate who is not receiving any shocks at all, but some 66 per cent of the subjects were deceived, and to the experimenter's dismay, 26 out of 40 (65 per cent) in the first experiment went to the highest possible shock level. (The behaviour of subjects in the experiments and their responses in the subsequent inquiry, to which Milgram devotes two chapters, make fascinating if disturbing reading.) From this basic situation Milgram derives eighteen experiments investigating the effects of varying different parameters – feedback from the victim, general environment, allowing the subject to choose the shock level, apparent status of experimenter, conflict of experimenters, etc. – which reveal how powerful some of these are: for example to achieve sizeable obedience effects it is necessary that the victim be to some extent remote (say in another room), and that the experimenter be present (otherwise subjects disobey, giving weaker shocks than instructed, and lie about it afterwards) and perceived as an authority figure (subjects simply refuse to obey experimenters whom they believe to be other subjects). Interesting, indeed fascinating, though some of these experiments are, they inevitably constitute no more than a beginning of research into this topic.

The least satisfactory part of the book lies in the explanation of obedience. Whilst Milgram disposes quite easily of the alternative hypothesis that the experiment simply permitted the subjects to release their aggression, his own theory, that subjects obey because they are in an 'agentic state', seems no more than circular, since an 'agentic state' is defined, essentially, as one in which subjects obey. He does, however, deal very satisfactorily with the essential ethical paradox of the work, namely that to investigate why people will inflict pain on others to order, the experimenter places them in a situation of ethical conflict which many found extremely painful: the answer is that only a very small proportion of subjects subsequently expressed negative feelings towards the experiment, a large majority indicating that they had learned something of personal importance and feeling that more such experiments should be carried out. It is a pity, but inevitable, that publication in book form as well as in the obscurity of scientific journals, although desirable in itself, will have so thoroughly 'blown' the experimenter's deception that experiments using this technique will be very much more difficult in future.

J. P. N. PHILLIPS

The Science and Politics of IQ. By LEON J. KAMIN. New York: Wiley. 1974. Pp. vii + 183. £5.90.

My first reaction on reading this book rapidly and with horrified fascination was to find myself saying, 'the boy stood on the burning deck'. A careful re-reading has not changed this reaction. So, what is the book about and, perhaps also, who has written it?

Leon J. Kamin is a Professor at Princeton University and in his acknowledgments indicates that he has had time off from a 'conditioning laboratory' to write this book. He ends the book with a long quotation from John B. Watson which summarizes Kamin's point of view about the importance of the environment on children's development. The whole book is 'concerned with a single major question: are scores on intelligence tests (IQs) heritable?'. To answer this question he reviews in detail the evidence on which the positive answer has been based, as well as some investigations which have 'slipped out of' the usual reference lists, and comes to the conclusion that 'there exist no data which should lead a prudent man to accept the hypothesis that IQ test scores are in any degree heritable' (p. 1).

But then he asks a supplementary question. If his conclusion is correct, how was it possible for so many psychologists, past and present, to believe the opposite? He deals with this supplementary question in chapters 1 and 2, 'The Pioneers of IQ Testing in America' and 'Psychology and the Immigrant'. They are made up largely of quotations from Terman, Bringham, Yerkes and others about the genetic inferiority of the 'lower orders' of society and of immigrants from certain parts of Europe, mostly dated between 1885 and 1923. Perhaps as a conditioning theorist, Kamin might have made allowances for the extent to which Terman and the rest were limited in their thinking by the times in which they lived. Their intentions were surely not malicious nor part of a conspiracy to keep the poor in their place at the bottom of society.

In chapters 3 to 5 he gets down to the task of reviewing the evidence on which the heritability calculations are based. He covers the investigations on separated MZ twins (chapter 3), the kinship correlations (chapter 4) and adopted children (chapter 5), especially those used by Jensen and Vandenberg. He deals in great detail, with papers by Burt, Shields, Newman Freeman & Holzinger, Burks, Skodak and Skeels, and others.

It is a very thorough examination indeed, using the original data where it is available to recalculate correlations, to show the evidence of environmental differences, for highly selective placement of adopted children and for inconsistencies of figures from one paper to another. The end result of all this is to make all the data on which calculations of heritability of IQ by Burt and particularly Jensen (who has had far more public effect) quite seriously suspect. This is very disconcerting for a reviewer who has read most of these papers, including Burks', in a far less critical way. It would be about a year's work to go over them all again, to check the validity of Kamin's criticisms and his reanalysis of the data. He deals also with some investigations where the results do not support the hereditarian hypothesis, and which, he says, have been omitted by Jensen and others. He seems to be slightly less rigorous in his examination of these neglected studies than of the often quoted ones.

Chapter 6 he devotes to an examination of 'The Accuracy of Secondary Sources' and gives some examples of ways in which the data and the conclusions of some researches have been distorted in the secondary source. A notable example is Jensen's use of the Fraser–Roberts data on the sibs of imbecile and feeble-minded children which Kamin discusses at length.

Chapter 7, 'IQ in the Uterus', deals with the available evidence about a point made by Jensen that differences in intra-uterine environment (showing in different birth weights) of MZ twins might account for differences in IQ even for twins reared together. He concludes that the evidence is not firm enough to show whether or not IQ and birthweight are related.

The final chapter (Conclusion) is very brief. The main points should be quoted: 'There are no data sufficient for us to reject the hypotheses that differences in the way in which people answer the questions asked by testers are determined by their palpably different life experiences' (p. 175). The phrase 'the questions asked by testers' leads him off to an attack on the Terman–Merrill and other tests and on the values and attitudes of the early testers. However, he adds that 'there may well be genetically determined differences among people in their cognitive and intellectual "capacities"' (p. 176) and that we have yet to find ways of measuring these properly.

Chapters 3 to 6 are the most important parts of this book, and need to be taken seriously.

Chapter 7 is important also, and perhaps should be kept in mind when we read elsewhere about the effects of 'minimal brain damage' on behaviour.

It may be difficult to make a really thorough critical examination of other people's research methodology and data without giving the impression of making a personal attack on the people concerned. (I remember with some uneasiness two highly critical book reviews that I have written in the past.)

Kamin's book would have been pleasanter to read if he could have left out chapters 1 and 2, and some of the personally directed remarks in other chapters. He gives also an impression that no psychologists except behaviourists, old and new, have seen the true light. He might find some evidence of enlightenment elsewhere; for example in Bruner's paragraph about a 'culture of despair' in *The Growth of Competence*.* Kamin is strongly critical throughout his book of the biasing effect of a commitment to the hereditarian theory on the selection and interpretation of data; he should also show that he is equally aware of the possible effects of a commitment to the other theory.

In a recent review of Jensen's collection of papers, *Educational Differences*,† I wrote that I held to an obstinate conviction that Jensen was wrong. Kamin at least provides some justification for my obstinacy. The schoolboy version of Casabianca concludes 'when his feet were burned away, he stood upon his head'. I hope I have not quite done this, though Kamin seems to have started in that position.

AGNES CRAWFORD

Psychology and the Political Experience. By ALAN HUGHES. Cambridge University Press. 1975. Pp. ix + 225. £4.40.

Dr Hughes concerns himself with four 'psychological dispositions': alienation, authoritarianism, ethnocentrism and anxiety. His book deals with problems in the measurement of these variables, the relationships among them, and the way in which they connect with political attitudes. Because, he argues, the first three of these have not been properly measured in the past, he undertakes to develop his own questionnaires to assess them, using a paired comparison format to combat acquiescence response bias. For the fourth factor, anxiety, he is content to use Cattell's scale from the IPAT Self-Analysis Form.

Having satisfied himself that he is measuring his personality constructs adequately, Dr Hughes proceeds to investigate the connexions between them. He concludes that alienation (conceived as social estrangement) is a partial antecedent of anxiety and ethnocentrism. Next he investigates the structure of opinions on a variety of social and political issues and identifies a number of reasonably independent factors, styled 'socio-economic radicalism', 'conscience radicalism', 'libertarianism' and 'defence leftism'.

Finally he connects up the personality factors with the political attitude factors. Somewhat unsurprisingly, authoritarianism was found to predict 'hawkishness' on questions of defence, anti-libertarianism and conservatism on matters of conscience (e.g. aid to Asia). Alienation was related to conscience conservatism and socio-economic radicalism.

His subjects throughout are urban Australians but he argues, quite reasonably, that the results should have relevance to any advanced industrial society.

The main weakness of Dr Hughes' book lies in its failure to connect with previous work in the field of political psychology. Eysencks' *Psychology of Politics* (1954) is cursorily dismissed as 'heavily reductionist'. Charles Hampden-Turner's book *Radical Man* (1970) is nowhere mentioned, nor is my own book *The Psychology of Conservatism* (1973). Since many of Hughes' ideas and findings are very similar to those expressed in these earlier works it is charitable to assume that he had not read them at the time of writing his own book.

The most interesting and original contributions are to be found in his analysis of individual case studies and political conversations in small groups and in his application of the four personality dispositions to the rise of Nazism in Germany in the 1930s.

A central argument of the book is methodological, that 'statistical techniques must be married to a human understanding, before they can be used to display the subject in all its intricacy'. His juxtaposition of hard data with impressionistic observation is quite refreshing and specialists in the area may find the book worth reading from this point of view. GLENN WILSON

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* BRUNER, J. S. & CONNOLLY, K., *The Growth of Competence*.

† CRAWFORD, A. (1974). Review of A. R. Jensen, *Educational Differences*. *Br. J. Psychol.* 65, 553–4.

The Psychology of Politics. By W. F. STONE. London: Collier Macmillan. 1974. Pp. ix + 291. £5.50.

This book begins with a description of the socialist commune set up by John Noyes at Oneida in New York State in the middle of the last century. The chief features of the commune were that no individual was allowed either personal property or a particular interest in any member of the opposite sex. Those desiring sexual congress had to make application to the ruling council, which vetted the application and only granted it on condition that no unseemly affections were allowed to develop. The commune encountered many problems; for example, who was to deflower the young girls coming into adolescence? Initially, John Noyes undertook to shoulder this burden himself. Later he apparently found it too onerous and delegated it to others. The commune lasted for about thirty years and then Noyes decided to leave and emigrated to Canada. Thereupon the community disintegrated and its members took up bourgeois family life. This is the initial puzzle posed by the author: how did such a satisfying way of life come to collapse?

It cannot be said that any clear answer emerges in Dr Stone's book. Nevertheless, he takes the reader through a pleasantly written discussion of theories of political behaviour and attitudes, including the authoritarianism personality, tough- and tender-mindedness, machiavellianism, liberalism, conservatism and the psychology of participation. This is a useful review of most of the major topics in the psychology of politics. R. LYNN

Perspectives on Social Power. Edited by J. T. TEDESCHI. Chicago: Aldine. 1974. Pp. x + 427. £7.50.

The University of New York at Albany has held annual symposia on the subjects of power and influence since 1971 and the papers presented at the first two of these symposia are published in this volume. Some of the more important problems discussed include the origin of power drives, the bases of power, cross-cultural studies of power relationships, economic power and models of conflict.

Publications of conference papers generally lack coherence because of the widely different approaches taken by the different authors, and tend to leave the reader dissatisfied because no single theory is fully worked out. The present volume is no exception to this generalization. Nevertheless, it should prove a useful introduction to the contemporary literature on the social psychology of power. R. LYNN

Psychology in Industrial Organizations. By L. SIEGEL and I. M. LANE. Illinois: R. D. Irwin. 1974. 3rd edition. Pp. xvi + 556. $12.50.

This textbook, the third edition since 1962, indicates right away that the authors are keeping abreast with new developments in industrial psychology. The new title also (in place of *Industrial Psychology*, 2nd edition), reflects an awareness of recent marked shifts in emphasis in the field. Perusal of the book quickly justifies expectations in regard to both.

The authors in their preface to the new text modestly claim that they are aiming to provide a comprehensive, connected overview of psychology in the industrial scene mainly for the undergraduate student and for the assistance of his teacher. In this, it could be fairly claimed, they have succeeded. Perhaps their success is due largely to the 'bridges' which they have built between the *general* psychology content of the text and the *industrial* psychology content. As a result, there is very little of the impression of 'bitterness' which one gets on reading many 'across-the-board' industrial psychology text-books.

Looking at just how the authors have changed the body of the previous edition to bring the new edition into line with what they regard as important shifts in emphasis in the different areas of industrial psychology, it is found that the one chapter on 'Organizational Theories and Structures' has been expanded to five (of the total of 20 chapters in the book), while the section on 'Personnel Psychology' (selection, job analysis, training) has been reduced, regrettably to this reviewer at least, in the cases of job analysis and training, from seven chapters to five. In this latter part of the new edition, however, there has been a welcome expansion, if a modest one, in the coverage of testing and training of disabled people.

On the side of general design of the textbook, retention of the two introductory chapters on scope and method, and the supplementary chapter on statistical computation is to be commended, particularly from the point of view of the intended main readership.

With regard to the studies quoted and examined in the different parts of the book, it is gratifying to see important research on this side of the Atlantic not being ignored as in many U.S. texts. For instance, the Tavistock coal-mining studies of Trist and Bamforth figure as a main contribution in part IV, 'Human Relations Psychology'.

For a text-book, the style is lively and very readable without lapsing at any point into superficiality. And occasionally the other point of view is wryly put. For example, in a context of the chapter, 'Effective Group Functioning', the undergraduate reader will get to know (like his elders) that a camel has been described as a horse designed by a committee.

At present-day prices the book is not expensive. Indeed, as a book – paper, printing, binding, format – it is a tasteful production and a good 'buy'.

The authors of this new edition have been so modest in their stated aims and so judicious-minded about how likely they are to succeed in these, that perhaps they should have the final word: 'We are under no delusion that this book will satisfy all psychologists with respect to breadth of coverage, depth of treatment, and topical emphases. However, we hopefully anticipate that the book will contribute effectively to the instructional partnership formed by teacher and author.'

JAMES G. M'COMISKY

Consumer Behaviour: a Cognitive Orientation. By R. J. MARKIN, JR. London: Collier Macmillan. 1974. Pp. xii + 576. £5.95.

The major objective of Professor Markin's book is to show how research on consumer behaviour may be used to do a better marketing job. To reach this goal he has produced what amounts to a text-book of social psychology written from an 'information processing' point of view. In Professor Markin's own words: 'Emphasis is upon consumer decision processes as a series of activities related to the purchase and consumption of goods, namely, problem recognition, search, evaluation, commitment and postpurchase consideration. All of these factors involve information processing activities. Thus information processing is seen as applied to many phenomena often treated quite separately in the literature of consumer behaviour, among them attitude change, socialization, motivation, and so forth' (pp. vii–viii).

This is an ambitious enterprise, and one which needs to be carefully stage-managed if it is to be successful. It is therefore particularly unfortunate that the book gets off to such a bad start. Broadly speaking, chapters 1 and 2 elaborate the concepts of 'market' and 'marketing'. A good deal of space is devoted to 'consumerism', meaning 'the social and economic movement of consumers, dissatisfied, disillusioned, and disenchanted with the efforts and activities of manufacturers, marketers, and researchers' (p. 19). It may be reassuring to learn, at the end of chapter 2, that in future, 'Marketers...must pay increasing heed to the Jeremiahs of consumerism, who are crying, "I am a human being: do not fold, bend, mutilate, violate, or pollute me!"' – but it is hardly a useful introduction to the technical information which is to follow (fortunately, written in a very different style).

Chapter 3 outlines certain traditions of thinking about 'the basic nature of man' (p. 57). This sort of summary is extraordinarily difficult to write, but Professor Markin loses all one's sympathy when he announces (rather surprisingly) that clinical psychology, experimental psychology ('fundamentally concerned with drives and motives that stem from basic biological conditions, such as sexual drive, thirst, hunger, cold, heat, and so on') and psychoanalysis are the three 'most important' schools of psychological thought 'from the point of view of our interest in consumer behaviour' (p. 62). From here Professor Markin moves on to consider whether 'existentialism and its concepts...offer any insight to help explain...consumer behaviour' (p. 74). He concludes that 'consumer behaviour might very well be viewed within the framework of heuristic problem-solving' (p. 75). And so it might, but 76 pages of this sort of 'prologue' is not the most effective way to make the point.

Chapter 4 develops the theme of heuristic problem-solving by examining the models of consumer behaviour due to Andreason (1965), Nicosia (1966) and Howard & Sheth (1969). These models are quite outstanding in the field, and have served as 'standards and guides for nearly all those who aspire to "model" consumer behaviour' (p. 85). Most of this material will, I think, be new to psychologists, and I, for one, would have appreciated a much more detailed presentation than that given here (10 pages in all). No doubt, 'it is rare', as Professor Markin says, 'to find a single published study that has utilized, been based upon, or even been greatly influenced by any of the models mentioned' (p. 102), but this is more of a comment upon the nature of the experimental studies extant in the literature than upon the (admittedly) preliminary nature of the models themselves.

Professor Markin himself does little more than use the models to identify major classes of variable which are relevant to our understanding of consumer behaviour. He then attempts to summarize some of the basic psychological research dealing with these variables, showing how it is relevant to the study of consumer behaviour. On the whole I am inclined to think that this attempt is a failure – and for two main reasons. Firstly, the book includes too much material, some of it of very dubious relevance. (It is, for instance, just not sufficient to give an account of Homans' theory of social exchange on the grounds that, 'To the extent that consumer behaviour is social behaviour, the relevance of Homans' propositions seems assured'; p. 383.) Secondly, little effort is made to review the literature dealing directly with consumer behaviour, and to place that literature within the wider psychological perspective which is provided. This is the most disappointing feature of the book, and may persuade many psychologists not to buy it.

IAN E. MORLEY

An Introduction to Community Psychology. By MELVIN ZAX and GERALD A. SPECTER. New York and London: Wiley. 1974. Pp. vii + 496. £6.85.

Community psychology comes of age with the appearance of this – its first – textbook. Its only significant precursor, upon which the present authors draw fairly extensively, is the collection edited by Iscoe and Spielberger (1970). Zax and Specter begin by tracing the origins of community psychology. These include a growing awareness of the impact of the environment upon the problems faced by clinical psychology and psychiatry, the emergence of the public health model as a rival to the traditional medical model, increasing dissatisfaction with traditional functions, and the inadequacy of existing services to meet society's needs. Rather than following the traditional method of attempting to help a troubled individual, the community psychologist usually operates by instituting a *programme* designed to have an impact upon a group of people occupying some specified position within the social or physical environment. Both logically and historically, therefore, the first step in the evolution of community psychology is the development of such programmes. More than half the book is devoted to reviews of community programmes, the great majority of which are preventive in intent. The closing section of the book discusses the problems involved in the present and future development of community psychology, taking up various criticisms of the community approach, describing the training of community psychologists at various American centres, and considering some problems of research in this field.

The book is well organized and clearly written. There is no doubt of its value as a textbook for psychologists involved, or contemplating involvement, in community work. Undergraduates interested in the range of potential applications of psychology should also profit from reading it. The authors provide a much needed conceptual framework for community psychology, with which is integrated their critical discussion of a wide range of descriptive and evaluative literature. In moving beyond this general appraisal, however, it becomes difficult to disentangle one's reactions to the book from one's views concerning the community approach itself. Many fundamental and exciting questions are prompted by reading the book; most of them are discussed, but it is perhaps unrealistic to expect final, satisfactory answers at this time. For example, should the community psychologist be a consultant, enhancing the effectiveness of other professionals, or should he be a 'front-line' worker? How much reliance should be placed upon 'non-professionals' in community programmes? Should the community psychologist be a generalist, or should he be equipped with specific skills and knowledge? Is community psychology a part of clinical psychology, or a separate profession and discipline? Just how much validatory evidence is required before one institutes a community programme? After the first flush of enthusiasm which currently sustains the field has waned, will community psychology prove any more able to 'deliver the goods' in a cost-effective manner than the traditional mental health professions? How much of community psychology is old wine in new bottles? All these questions have obvious implications for the training and function of the community psychologist, and of clinical psychologists seeking to extend their role.

Community psychology as such is an American phenomenon. The programmes and courses described by Zax and Specter reflect their American context and origins. However, many of the factors precipitating their development in the United States are currently taking effect in Britain. We already have a few people explicitly employed as community psychologists. More covertly, many clinical psychologists are finding new roles beyond the purview of psychiatry. Zax and Specter's book will facilitate these developments. It is to be hoped that the establishment of adequate training programmes will precede any great increase in the numbers of community psychologists. Here in Britain this book will find its most significant use as a primer for such courses, presenting the American model as a basis or as a point of departure, for the training of community psychologists.

D. A. SHAPIRO

ISCOE, I. & SPIELBERGER, D. C. (eds.) (1970). *Community Psychology: Perspectives in Training and Research.* New York: Appleton-Century-Crofts.

Ethology and Psychiatry. Edited by N. F. WHITE. London: Bodes Canada Ltd. 1974. Pp. xi + 264. £1.90.

The study of animal behaviour in natural man and artificial situations, and drawing from it inferences applicable to human beings, is not only popular but respectable, and a work with so interesting a title is bound to excite attention.

Professor Norman White outlines the history of ethology and bravely tries to point out its relevance to psychiatry which, as he says, has always looked to other disciplines, be they anatomy, philosophy or chemistry for help. All the same, his paper of 20 pages seems somewhat over-documented, with 10 pages of references. Ten of the thirteen contributors come from North America and this doubtless accounts for the overall tortuous style and the tendency to use ten words where five would do.

It is irritating yet again to see unhappiness equated with depression, and indeed the elaborate and it would appear, cruel experiments which demonstrate that young monkeys are miserable when totally separated from their own kind and recover when company is restored to them, and that male rough-and-tumble play is inherited and not learnt, hardly seem to justify such iron-mongery and suffering. The facetious final sentence is a sad commentary.

Underlying more than one contribution is the question of whether the care of a mother for a child is more appropriate than that of the father. Because a father monkey helps to look after the tribe and the mother cares for the infant is not necessarily a reason to assume that man should not care for the home and woman go to work. Infant predators are relatively rare in human living. What unique qualities has the mother that the father lacks? Only two – she can lactate and she knows the baby is hers. Is the extended family really the best arrangement? All infants, however, have an innate predisposition towards 'attachment'. Is the 'new' Spock correct? Can forty million years of attachment to a single mother which is part of the inherent repertoire be overthrown at the demand of emancipated woman?

Marley, using as a basis the development of birdsong, suggests that constraints have been evolved which guide biologically important learning processes. His well-reasoned paper has one surprise. Was it unexpected that birds had a dominant hemisphere? Rosenblatt points out that the newborn of many species have two attachments – to the mother and to the nest, and inevitably in animals where the mother, in effect, is also the nest, a different form of development will follow.

The chapter on infancy in hunter-gatherers is good and so of course is Hinde's contribution. If sometimes the suspicion arises that certain authors had difficulty in bringing in psychiatry this is because the original symposium was on the relation betweeen ethology and human development.

The book is well produced, copiously illustrated, pleasant to hold and reasonably priced. It can be recommended to undergraduate readers in psychology and to young psychiatrists.

J. A. R. BICKFORD

Problems of Adolescents: Social and Psychological Approaches. Edited by R. E. Hardy and J. G. Cull. Springfield, Ill.: Thomas, 1974. Pp. xvi+278. $14.75.

Psychological and Vocational Rehabilitation of the Youthful Delinquent. Edited by R. E. Hardy and J. G. Cull. Springfield, Ill.: Thomas. 1974. Pp. xvii+248. $11.50.

The content of these books is only tangentially related to their titles: *Problems of Adolescents* is mainly about American delinquents and *Psychological and Vocational Rehabilitation of the Youthful Delinquent* does not tell the reader much about rehabilitation. The books are published together as part of the same series and edited by the same people, who also contribute some of the chapters. The two volumes are presumably meant to be bought together and studied together. It is therefore astonishing to find that out of a total of 28 chapters (15+13), they share 20; i.e. about 70% of their content is identical. Why any publisher or editor should want to do such an odd thing is beyond my charitable imagination.

The duplication of the chapters would matter less if in each book they contributed towards the building up of a different approach, but in neither volume does the material hang together to give a coherent picture. It is not only the quality that is uneven, though generally poor, but also the simple irrelevance of some of the chapters to the other chapters. In *Problems of Adolescents* there are five chapters, out of a total of thirteen, on issues related to drug abuse. Allowing for this heavy emphasis on the drug problem in adolescence, even by American standards, the picture that emerges from these chapters is sketchy in the extreme – you learn the chemical name for mescaline '(3,4,5-Trimethoxyphenylethylamine)' and the slang words for various substances, but precious little about why adolescents take drugs in the first place.

The major difficulty in evaluating these books is in not knowing who they are addressed to. In the prefaces the editors confidently recommend the fruits of their efforts to 'all professional personnel concerned with problems of adolescents' – including psychologists, psychiatrists and social workers, as well as to the student in the classroom. It takes enormous skill to pitch such a varied collection of chapters at the right level of discourse for such a motley crowd and I just do not think the editors have succeeded. Generally, the contributors avoid confronting the many theoretical issues in the particularly thorny fields of delinqency and adolescence. When they do, it is either through categorical assertion, e.g. 'No one is born more likely to become delinquent than anyone else,' or platitudes, e.g. 'juvenile delinquency must be seen in an environmental context and not solely as a characteristic of the individual'. Whilst the student will find neither volume a particularly good or comprehensive introduction to adolescence or rehabilitation of the delinquent youngster, he will nevertheless find some useful chapters on 'Families in Crisis' by G. A. Ingram, 'Counselling the Parent of the Chronic Delinquent' by J. Rosenberg, and an exceptionally perceptive, and chatty, paper on the problems of using 'College Students as Volunteers' by N. Manasa.

A psychologist, interested in assessment, treatment or research, will find little to whet his appetite and even less to nourish. There are altogether about a dozen loosely strung paragraphs on psychological evaluation where the only two tests mentioned are the TAT and the Bender Gestalt. A chapter on 'Behaviour Modifications and Delinquent Youth' is useful for a list of up-to-date references but is otherwise remarkably uninformative about its subject-matter.

Other trained 'professionals' are likely to be irritated by the diffuse focus, the loose conceptual framework, the general avoidance of theoretical issues and the low empirical content of the chapters. Most of the contributors are practitioners rather than academics, but this laudable departure from the usual pattern does not justify the almost total disregard of the distinguished work on both sides of the Atlantic on delinquency and adolescence. As 'impressionist' canvases on the subject of delinquency the books may pass muster, but a 'professional' does not need impressionistic pictures of his own field, and the student or the non-professional can find more imaginatively drawn, and much cheaper, portrayals elsewhere. MASUD HOGHUGHI

Sociological Aspects of Drug Dependence. By C. WINICK. Oxford: Blackwell Scientific Pp. 327. £21.00.

Even in these inflationary days, the feature of this book most likely to excite immediate comment is its price. Including the index and several sheets white as driven snow, the book contains 327 pages: the cost per page is therefore approximately 6.4 new pence. Not long ago the price would have suggested that we were in the rare book market rather than dealing with any new production. With a price like this one will have to calculate annual depreciation for tax purposes, as with a motor car. A resolute protest should be entered by a properly resistant buying public against publishing practices which so misserve the scientific community. The matter, in this instance, deserves rather special comment, for this book is of such excellence that it ought to be widely available to all manner of students and research workers. At its current price they will only find it in libraries, and many libraries which are not richly endowed will have to turn it down.

The excellence of this book stands out sharply against the general background of the drug literature of recent years – a literature which has become increasingly depressed and repetitive. Here we have a collection of papers remarkable for the standard of good writing, the attack on essentially important questions, and the level of freshness and intelligence. For anyone who in a more despairing moment has been tempted to conclude that sociology is all postures and obfuscations, this book will come as a cheerful corrective.

Winick himself contributes the first chapter, and presents a theory of the sociology of dependence. He makes much use of the concepts of role strain and role deprivation: his exposition would perhaps have benefited from sharper definition of these terms. As so often happens, one is left with the feeling that sociology unnecessarily handicaps itself by its determined disregard for psychology. Despite these reservations, Winick's essay must be seen as a major effort to provide a common basis for understanding such seemingly disparate happenings as the heroin epidemic in the U.S.A., the amphetamine epidemic in Japan, and an analgesic epidemic in Switzerland. There is a need sometimes for daring thinking which gets a little ahead of the data.

Some of the other papers in this book deal with the labelling process in alcoholism (a factual investigation and no mere speculative essay), breathe new life into discussion of drug education, intelligently dissect the influence of the mass media, and look at the broad social implications of a methadone maintenance programme which in the U.S.A. currently has 75,000 persons taking that drug. The final section of the last paper – 'Towards a radical program to combat the drug-crime syndrome' – is, in the American phrase, not exactly data-based: 'The costs for the social eradication of heroin addiction must be borne by those who profit most from this plague and who can afford to pay for this program: the large corporate enterprises.' A little bit much at 6.4 p the printed page.

GRIFFITH EDWARDS

Research Strategies in Psychotherapy. By E. S. BORDIN. London: Wiley. 1974. Pp. ix + 272. £9.15.

This book examines in great detail the methodological problems of research and the research results in virtually every aspect of psychotherapy. It is therefore a mine of references up to the early 1970s for scholars and students of clinical psychology.

Bordin claims that this book took him almost a decade to write. Certainly the extremely careful treatment each question receives indicates that this was time full spent. Thus, for example, we find separate sections in the methodology of research into the outcome of psychotherapy (a simple kind of research which Bordin does not recommend) on: history and maturation of patient during psychotherapy; the effects of observation; instrument decay (boredom of raters over the period of psychotherapy); statistical regression; definition of treatment and definition of populations. After all this discussion we are told that 'there is an infinitesimal probability that any one-shot research attempt will ever significantly advance our knowledge in this area', rather we must keep on 'plugging away at small bits of knowledge which only after an appreciable period of time might attain a higher order of significance'.

To your reviewer the last statements highlight both the strength and weakness of this book. Too often when we have the summaries of the researches in the various areas of psychotherapy and struggle to grasp the often complex problems of methodology, we end up with the statement that nothing much has yet been discovered and more and better research is needed. As Bordin says, we have a good understanding of the problems if not the solutions. Now this is unsatisfactory. Mountains strain in labour, etc. On the other hand, the strength of the book lies in the fact that Bordin has brought together and made coherent a vast array of research so that the little bits of knowledge often uninteresting in themselves do gradually begin to make sense.

As we have indicated, this book covers every aspect of research into psychotherapy. The fields covered include topics such as outcome and process, methods of simplification (laboratory analogues of the therapeutic situation), therapeutic invention and the change process, the personality of the therapist and the personality of the patient as an influence on psychotherapy. Such coverage ensures that few readers could take seriously any longer (valuable for behaviouristically inclined students) simple claims covering the efficacy or otherwise of this or that therapy – an important point still since Eysenck's old claim that psychotherapy is ineffective can still be heard.

Although great emphasis is placed on research design and method, many of the studies cited as evidence seem to your reviewer to be peculiarly weak in respect of measurement and reflect Bordin's basic interests as a clinician, who also recognizes the need for research, rather than a researcher. For example, in the chapter on the personality of the patient as an influence on psychotherapy we find references to personality tests such as the Personal Orientation Inventory, the Rorschach, the F scale, the MMPI, Levy Interpersonal Checklist, Blacky Pictures and the EPPS. Surely now the lessons of twenty years of factor analysis of personality should have been learned, namely that the major variables of personality at the second order are extraversion and anxiety and that most of the tests are hopelessly compounded of many different factors. It is like trying to measure abilities with no reference to the factor analytic findings whether Thurstonian or not.

In brief, this book gives a good account of most of the research problems in psychotherapy and would be most helpful to those wanting to undertake research in this area or simply wanting to know the almost current position. However, it is not as rigorous as this reviewer would like it to be and it shows a lack of interest in multivariate research procedures, some of which are ideal for the problems of research in psychotherapy.

PAUL KLINE

Advances in Psychobiology. Volume 2. Edited by G. NEWTON and A. H. RIESEN. New York: Wiley-Interscience. 1974. Pp. x + 330. £12.00.

The last few years have seen a massive increase in the number of publications in the area of the neurosciences. Apart from the attempt to keep abreast of a growing tide of research papers published in the specialist journals, the research scientist is exposed to a flood of more substantial review articles contained in series variously titled 'Handbook of...', 'International Reviews in...', 'Progress in...', 'Current Trends in...', etc. The present book is one such volume, containing eight chapters, most of which emphasize in part developmental aspects of psychobiology.

Those readers who are Spanish-speaking may already be familiar with Gorski's opening chapter dealing with neuroendocrine mechanisms involved in the development of sexual behaviour and its emergence at maturity. This is the translation of an article which originally appeared in *Neuroendocrinologia* (1973). I feel the editors are justified in bringing this clear exposition to the attention of a wider audience. The concept of sexual differentiation of the brain is now well established in the animal literature and may prove useful in understanding human sexuality.

The longest chapter is a review by Fifková on structural changes in visual centres following either long-term illumination or visual deprivation. Results of microscopic analysis of synaptic alterations are described for different levels of the visual pathway in rats. Her findings indicate that visual deprivation and increased illumination affect selectively different types of synaptic junctions between the cells in the retina. Visual deprivation, furthermore, appears to severely alter the histological appearance of the lateral geniculate body and visual cortex, while not affecting the superior colliculus and optic tract. I find it difficult in the absence of empirical evidence to accept her *ad hoc* suggestion (p. 85) that an animal that has only one eye functional would keep it open over a longer period of time than an animal having the use of both eyes.

The contribution by Zimmerman, Geist and Wise on the behavioural consequences of protein malnutrition completes a trio of larger chapters. The authors criticize earlier studies, which used mainly non-primate species, for confounding the nutritional variable with environmental deprivation. Using infant rhesus monkeys and subjecting them to carefully controlled combinations of diet and social experience, Zimmermann *et al.* suggest that malnutrition may not produce deficiencies in cognitive or learning tasks *per se*, but rather impair selective attention mechanisms. In addition, low-protein diets in early life appeared to contribute to the development of behaviour that was maladaptive to social interactions. The authors propose that early environmental deprivation and protein malnutrition during infancy produce similar behavioural malfunctions.

The fourth and fifth chapters both deal with physiological aspects of memory storage. The larger of the two, by Gold, Zornetzer and McGaugh, examines the memory disruption caused by electrical stimulation of the brain. This is one of several recently published reviews from the prolific group led by McGaugh at Irvine, California. There is considerable overlap between some of these reviews, and one wonders whether or not the editors of the various collections in which they have been included are aware of the possible redundant nature of the literature being covered. In spite of this uneasiness, the chapter is highly readable and poses important questions about the ECS parameters which produce retrograde amnesia (RA) and the possible neural correlates of RA. From their comments, we cannot expect the answers to such questions to be simple. The short chapter by Gaito contains mainly historical material and will be of little use except to those unfamiliar with the biochemical approach to learning and memory.

The final three chapters are all concerned with brain function in perception. Tee and Riesen show that the failure of many species to learn a mirror-image discrimination may not necessarily be a failure to differentiate the stimuli. The difficulty with mirror-image tasks may lie not in the perceptual analysis, as suggested by theorists such as Deutsch, Dodwell and Sutherland, but rather in the memory-encoding process.

The significance of White's chapter on visually evoked responses produced by patterned stimuli lies in the possible applicability of the technique to clinical diagnosis, and developmental study of the visual capacities of human infants and young animals. The final contribution by Jones is a summary of many in-depth interviews with drug users of various types. Phenomenological reports of this kind may be useful in directing experimental effort towards research strategies aimed at uncovering how these drugs act on the nervous system.

The editors in their preface to this series state that it will be 'international as well an interdisciplinary in scope'. This volume certainly fulfils the latter criterion. International it is not. Twelve of the 13 contributors are attached to laboratories in the USA (9 of them are in California). The exception is John Gaito – from Toronto, Canada. This is a volume for the library rather than the individual bookshelf.

KENNETH BROWN

Basic Structure and Function in the Nervous System. Edited by D. G. Stein and J. J. Rosen. New York: Macmillan. 1974. Pp. x + 246. £2.25.

Motivation and Emotion. Edited by D. G. Stein and J. J. Rosen. New York: Macmillan. 1974. Pp. viii + 200. £2.25.

Learning and Memory. Edited by D. G. Stein and J. J. Rosen, New York: Macmillan. 1974. Pp. viii + 208. £2.25.

Despite the somewhat pretentious overall title given to this short series of books ('Neural Organization and Behavior: a Systems Analysis Approach to Brain Function)', it consists simply of another collection of papers on the general subject of physiological psychology. As such, it seems appropriate to consider three questions: (i) Are the individual papers among the most significant and influential in their fields? (ii) Is the collection as a whole representative of the general subject of physiological psychology, or, at least, of the subject-matter of individual courses contributing to degrees in psychology? (iii) Do the editors' introductory material, the organization or orientation of the readings, or any other characteristics of the collections as a whole contribute any novel insights or conceptual advances?

Answers to the first two of these questions must involve evaluation of the significance of achievements and trends in the field. A necessarily brief description of the contents of these three volumes will allow the potential customer some chance to apply his own value judgements. Each volume contains between 15 and 18 papers grouped into two or three sections. Each section has a short introduction by the editors, attempting to set the papers into a setting and conceptual framework, and is concluded by one or two 'study questions', requiring the student to integrate the various material by applying it to his thinking about a particular problem. The first volume is divided into sections on 'Neural Function: Flow of Information in the CNS' (three papers, by Bullock, Hodgkin and Wachtel and Kandel, on basic neurophysiology, and one, by Gonatas, on neurological abnormalities in a retardate), 'Transduction and Integration of Sensory Input: Coding, Decoding, and Scanning Mechanisms in the CNS' (contributions by Kennedy, Erickson, Whitfield and Hubel on coding in different sensory systems, as well as by Robinson on eye-movement control and Sprague on interrelationships between superior colliculus and cortex) and 'Arousal and Attention' (Jouvet, Scheibel and Scheibel, Detusch and Deutsch, Hernández-Peón, Glaser and Griffin, Hubel *et al.*, Adametz and Purpura on various aspects of sleep, arousal, habituation and selective attention). Part I of the second book is concerned with 'Motivation' (Stellar's classic theoretical paper, Hamilton and Brobeck, Schwartzbaum, and Teitelbaum and Epstein on the effects of lesions on feeding, Valenstein and Valenstein, Miller, Valenstein *et al.*, and Wise on the motivational properties of electrical and chemical stimulation) and part II deals with 'Emotional Behaviour' (Papez on the neuroanatomy, Molina and Hunsperger, Grossman, Jeeves and Rosvold *et al.* on the effects of manipulation of various parts of the limbic system and related structures, with Denenberg and Whimbey and Schachter on experiential and cognitive variables affecting motivational behaviour). The third volume's sections are headed 'Learning: Anatomical Aspects' (Mishkin *et al.*, Chow and Randall, Kimble and Lashley on the effects of various lesions on learning and recall, with critical methodological and conceptual papers by Stein *et al.* and Gregory), 'Learning: Electrophysiological Aspects' (empirical papers describing EEG recordings by John and Killam and by Chow, with methodological and conceptual contributions from Fox and O'Brien, and Pinneo) and 'Memory: Information Transfer and Storage' (biochemically orientated, with Babich *et al.*, Frank *et al.*, and Luttges *et al.*, on the RNA controversy and Flexner *et al.*, Banks and Russell, Deutsch, and McGaugh on various pharmacological manipulations and their effects on memory processes).

It can be seen that the subject matter of this collection is fairly representative of the traditional fields of physiological psychology as defined by the standard American text-books, and very few of the papers would seem to be either trivial or too esoteric in significance to be worth a student's while to read. Quite a good balance is struck, over the collection as a whole, between purely experimental accounts, reviews, and critical or theoretical papers, although this balance is not always maintained in individual sections. The section on sensory coding achieves a breadth and comprehensiveness, not present elsewhere, by the inclusion of four review articles, each set within a clear conceptual framework. On the other hand, the selection of purely experimental papers on the possible role of RNA in memory fails to give this coherence but serves better to give a feel for the technical problems involved in real research and to illustrate the confusing and often, apparently, contradictory implications of work in this area. Again, the empirical accounts in 'Learning: Anatomical Considerations' are tempered by methodological critiques, but, as with the RNA and memory section, in the absence of any papers giving an overview, the student may find it hard to see the wood for the trees. The volume on 'Motivation and Emotion' seems the least coherent. The individual contributions mostly consist of descriptions of widely known phenomena and the two conceptual papers (by Stellar and Papez), immensely influential in their time, are now of largely historical interest. To some extent, the limitations reflect those of research in these areas in that convincing theoretical formulations are not available. But the undue emphasis on papers concerned with the control of food intake and with traditional approaches to limbic system function could usefully have been complemented by some on other primary drives, and more space might have been given to recent work indicating some of the limitations of classical ideas of hypothalamic organization.

These reservations are, however, matters of detail, and no selection would preclude some such quibbles. To summarize the relevance and representativeness of the contents, suffice it to say that more than a third of the readings included are, or are closely similar alternatives to, references actually given by the present reviewer to his students for a fairly conventional (in basic subject-matter, at least) course on physiological psychology – a high proportion when the quantity of available literature and the variety of alternatives are considered. The only major regrets concern the omission of any material on subjects that tend to be regarded as peripheral by the American text-book definition of physiological psychology but which are of real interest and relevance to students of psychology in general, most particularly on human brain damage and neurology, on motor control and on psychopharmacology.

As the title to the whole series suggests, an attempt is made by the editors, in their introductory material, to present a distinctive point of view, a conceptual framework concerning the basic nature of neural organization. But they do not define the nature of their 'systems approach' nor describe the distinctive conclusions to which it leads; and the repeated statements about the interrelationships between systems and the 'hierarchical organization' of these are too vaguely and abstractly stated to be meaningful. In some of the section introductions, particular conceptual problems applying to individual subject areas are usefully and clearly defined, and one refreshing characteristic of the selection of papers is the number that are critical of current approaches and ideas. A genuinely original or insightful contribution to thinking about the brain would, however, have required a much more detailed, thorough and explicit exposition of the editors' theoretical orientation. Enough thoughtful comments are included in the introductory material to suggest that this might have proved worthwhile. In its absence, this series must be considered simply as a series of readings to supplement a course. As such, it is probably as representative and useful as most such collections and a good deal more so than some. The standard of production and illustration is high, though this has, of course, to be paid for.

R. I. HORRELL

Control Theory in Biology and Experimental Psychology. By Frederick Toates. London: Hutchinson Educational. 1975. Pp. 264. £6.50.

The applications of control theory concepts in psychology have been fewer than was hoped in the heyday of cybernetics. However, several of the applications have been highly successful; in particular, eye-tracking movements and homeostatic control of food and water intake. The reason for this success appears to be partly the greater ease of identifying the parameters obtained from frequency-response or differential equation descriptions of behaviour than is met with, for example, in manual pursuit-tracking. As a contributor to both of these fields, Toates is well qualified to write an introduction to biological applications of control theory. He leads the reader through some simple mathematics and slips in the Laplace transform and the Nyquist plot before we have time to notice. He then shows how a detailed and precise application of control theory simulations to eating and drinking behaviour can summarize in a satisfactory fashion the results of a burgeoning number of experiments. This is the declared justification for writing the book; the careful introduction means that we ought to be able to assume that control theory is a part of the theoretical armamentarium of the rising generation of physiological and motor-skill psychologists.

MARK HAGGARD

Obese Humans and Rats. By S. Schachter and Judith Rodin. Chichester: Wiley. 1974. Pp. ix + 182. £6.00.

Schachter's earlier theory is very well known: the obese are sensitive to external cues related to feeding and insensitive to internal cues from nutritional physiology. Schachter and Rodin present a considerably modified version: obese people are more reactive than lean people to highly salient environmental stimuli.

The scientifically strongest parts of the book are chapters on the experiments giving rise to the revision. The addition of a reference to the prominence of the food-related external cues was forced by Lee Ross's and W. G. Johnson's Ph.D. work: their overweight subjects ate more and worked harder than normal-weight subjects when the food was highly visible, and ate less when food was very poorly illuminated. An extension of the theory to environmental stimuli and performances of any type, not just cues and behaviour related to food, is substantiated by Patricia Pliner's findings, mostly already published. Some effects unrelated to food and feeding are also provided by Rodin, but Pliner shows specifically that overweight subjects react abnormally strongly to high salience (whether unpleasant or pleasant) and under-react to low salience.

Despite many passages of exposition, there are some notable gaps in the specification of the current version of the theory. Assuming that internal cues are still regarded as relatively ineffective in obesity (a point not explicitly restated here for man), it is unclear whether this is because the cues are low in salience or because they are not external. Another issue for either the present saliency formulation or the original external–internal formulation, and as much also for closely related distinctions such as field dependency or focal and peripheral attention, is whether the differences are basically relative or are primarily an absolute shift at only one end of the polarity with secondary effects at the other end – an experimentally almost impossible question.

Perhaps the revised theory can dispense with the external–internal distinction as an explanatory category. That would have considerable advantages. For example, why is taste classified as external and sensations possibly arising from the stomach as internal? More important, the distinction is simply unusable to the extent that the behavioural importance of the external stimulus in food-related behaviour depends on the learning of contingencies between the external stimulus and internal after-effects of feeding. Such learning has been demonstrated and it may even dominate hungry behaviour.

The data have methodological limitations. Dietary self-report is dismissed and then used. Confounds are identified and then permitted in a new experiment. Throughout the book, 'obese humans' are almost always overweight American students. No mention is made of the problem of the lean but overweight football player. The nearest measure to a skin-fold comes in a small experiment using Schachter's imaginative style of field study, in which customers in a Chinese restaurant (of interest for their willingness to work at using chopsticks) were classified by eye as normal, chubby or obese. On the other hand, the experimental groups are usefully large and relatively unselected for aetiology or consequences of overweight.

Interleaved with the accounts of human studies, there are reviews of the literature on rats (and sometimes other animals) with lesions in the ventromedial hypothalamus. Many parallels are drawn between VMH rats and obese people. Sometimes the only rationale for a human experiment is the search for a parallel with the VMH syndrome. The authors apologetically call this 'science by analogy' but do not make clear why they persist with it none the less. A mere analogy only creates experiments on the analogy, which either support it and gain nothing or break it and lose all. Human obesity research is not so short of testable theories that it needs experimental inspiration from other sources. There is also the temptation to move from the analogy to the theory that the obese have VMH lesions, a *non sequitur* which generates no obvious experiments and is likely to fortify unfortunate attitudes in prognosis for obesity. If the analogy alone has any implications for treatment, they are not mentioned here. The analogy is put to one theoretical use: it is extended to the dynamic hyperphagic phase early after the destruction of the VMH, to see whether the abnormal behaviour in the later static phase is a consequence of obesity rather than, as hoped, a cause of it. Unfortunately this approach to an important issue relies on the false assumption that the dynamic VMH rat is lean: in fact, the animal is merely less obese than in the static phase.

The analogy as such may therefore be unhelpful. However, some of the theoretical issues in Schachter's approach to human obesity do have logical parallels and historical connexions with the controversies over VMH obesity. The classical interpretation of the VMH syndrome was very similar to the original version of Schachter's theory – the rats were insensitive to internal satiety cues and were hypersensitive to food cues. This interpretation is now widely acknowledged to be ill-founded. The Graff and Stellar paper cited in the book shows that obesity and finickiness can be dissociated in the ventromedial hypothalamic region. More clearly, VMH rats have nothing wrong with satiety itself: they over eat when food is freely available because the VMH normally restrains peripheral food-processing mechanisms which divert nutrients away from satiety mechanisms, and they are under-motivated unless starved to lean body composition because their excess fat can be broken down to reduce interest in food. (Incidentally, similar mechanisms could give obese people weak internal cues, so in fact their sensitivity is normal.) It remains to be seen whether current views of the VMH syndrome will settle down into good analogies for Schachter's revised theory of human obesity. Over-emotionality may be common to the two conditions, as discussed in the book.

The authors successfully sort out a number of artifacts and false comparisons between animal and human behaviour. However, one of the analogies they cite repeatedly – a lower meal frequency in the obese – is in the rats probably itself an artifact arising from another of their analogies, namely the hard-to-explain differences in effects of liquid and solid foods: the early data showing fewer meals in VMH rats were for liquid diets, whereas more recent work was using more natural solid foods shows no such clear effect.

Those who doubt whether brain damage can tell us anything about physiological processing except when we already have a well-constructed theory based on the behaviour and physiology of normal organisms will regret the attention paid to the VMH 'puzzle preparation'. Furthermore, even if the analogy holds, there are many mechanisms other than the brain pathology for generating in human obesity some phenomena similar to the VMH syndrome.

D. A. BOOTH

New Approaches in Psychological Measurement. Edited by P. KLINE. Chichester: Wiley. 1973. Pp. ix + 269. £5.50.

This book is yet another example of the current vogue for publishing a collection of contributions from several authors in the same volume. One may only assume, albeit erroneously, that the selection of authors and their content somehow reflects the basic outlook of the editor. Evidently this method of producing a book must be both inexpensive and speedy without committing the editor to any specific standpoint. It is perhaps symptomatic of the prevailing bland, non-contentious, liberal intellectual climate in which we live. Professor Butcher makes reference to this phenomenon in his contribution, 'Intelligence and Creativity' when referring to Arthur Jensen.

Thus the reader is presented with the contributions of nine authors in eight chapters. The book is held together by an introduction and 'concluding remarks' totalling just over two pages and written in the first person plural – 'we' – so as to obscure the editor's personal viewpoint.

The opening chapter by Professor Philip Levy is perhaps the most stimulating. He clearly sets out those questions every applied psychologist ought to be asking himself every time he employs a psychometric test. Levy argues that psychometric devices should be used to test hypotheses derived from observation rather than the normative test results serving as a generator for the production of a plethora of hypotheses. It is a chapter that merits inclusion as prescribed reading for all clinical and educational psychologists in training.

The remaining contributions are of the literature survey type. J. D. Hundleby states: 'in the area of objective tests of personality...the single test will disappear...'. This is hardly surprising. The concept of personality has itself all but evaporated from my personal conceptual system. No doubt the tests in my cupboard will take rather longer to turn to dust. Semeonoff's chapter, 'New Developments in Projective Testing', is discursive but oddly out of date when he speaks of the contribution of psychometrics to diagnosis and therapy. He suggests that projective techniques might be employed on the grounds of clinical expedience rather than claiming scientific validity, but he is never specific about the kinds of questions these tests might answer.

In contrast Jasper Holley presents a sophisticated statistical technique in his chapter, 'Rorschach Analysis', which demonstrates that Rorschach responses can validly predict psychiatrists' discriminations between normals, neurotics and schizophrenics. One wonders why he bothered, though the statistical method bears further examination. Bannister tells us little new in his chapter, 'Evaluating the Person'. What is not said is that the interpretation of the grid matrix produced by any particular patient is as much a projective test for the psychologist as any TAT ever was. Bannister's validity safeguard would seem to be that he conveys the tentative grid analysis to the patient to see if he can make sense of it.

The remaining contributions represent a degree of academic effort in that a great number of relevant publications are surveyed. Paul Kline in his chapter, 'Assessment in Psychodynamic Psychology', concludes that 'psychometrists with imagination and insight provide sound quantification into psychodynamic psychology...' This reviewer clearly is unimaginative and without insight.

The Levy contribution apart, this book seems hardly worth £5.50.

ALAN DABBS

Experimental Design and Analysis. By W. LEE. Reading, England: W. H. Freeman. 1975. Pp. xvi + 353. £7.80; and *Workbook for Experimental Design and Analysis*. Pp. 116. £1.60.

This book, together with its accompanying workbook, offers to non-mathematical behavioural scientists a practical introduction to multifactor experimental design and analysis of variance. It marks an important advance on previous texts in this field in that it shows how the non-mathematician can create his own designs and analyses to fit the specific demands of his research. This is a fundamental problem which previous authors have largely ignored either because they were unaware of its existence or because they had no solution to offer.

The problem arose from the rapid development of multifactorial analyses of variance designs whose proliferation and apparent mathematical complexity led to many text-books which concentrated on computational instruction of basic standardized designs. This forced many research psychologists to adopt one or other of two basic strategies.

The first of these strategies was to select one or more basic designs for which they had learned the use of the computing formulae, and then force the research problem to fit the design. This clearly leads to a severe restriction in the type and complexity of problems with which the researcher can deal. The other strategy simply ignores the basic assumptions of different designs and analyses with the result that statistical calculations are carried out on data which fail to satisfy the necessary statistical assumptions. The first leads to the testing of trivial hypotheses by elegant statistical methods, the second leads to pseudo-science in which sheer speculation and laboratory anecdotes are given a gloss of statistical jargon. Papers illustrating one or other of these strategies predominate in current journals.

A text which can free psychologists from these constrictions by showing how standard designs can be altered or enlarged and new designs created without requiring the reader to invest in considerably increased mathematical sophistication is clearly welcome, and this text meets this criterion. Its only prerequisite is the usual short course in introductory statistics covering sampling and probability distributions, estimation of parameters and simple hypothesis-testing and, in any case, after a short introduction the book devotes a chapter to a review of these basic procedures.

The ground rules for acquiring the skill of creating one's own designs, deriving correct computing formulae and choosing appropriate F ratios are discussed in four chapters which constitute the heart of the text. The first discusses the several types of factors which enter into experimental designs, the relationships among these factors and a language in which similarities and differences of structure can be symbolized. The second shows how models may be derived from these symbolizations; the general model used is a score-model, i.e. one in which a score on a criterion variable is equated with main effects, interaction effects and errors.

The third gives general rules for calculating relevant sums of squares, including as a unique feature a set of sum of squares elements tables for facilitating computation, and the fourth covers rules for defining and choosing correct F ratios. The remaining three chapters exploit the advantage of this approach in relation to conventional problems – Latin square designs, missing scores, analysis of of covariance, multiple comparisons, power computations and trend analysis. In this way the majority of multifactor designs commonly used by psychologists are included, with the advantage that guidance is given for creating new and better designs when the research project requires them. The book is thus more comprehensive than any other treatment of designs having factors related by crossing and nesting; the price of this specialized treatment is that non-parametric methods, quasi-experimental designs and multivariate analysis of variance are not treated at all. In addition to the usual statistical tables, the appendix includes Feldt–Mahmoud and Pearson–Hartley power charts. The workbook provides tests of understanding of the the text and numerical exercises; all the latter involve unrealistically small numbers of scores on the grounds that the logic followed in obtaining the answer is more important than computational practice or initial exposure to real data.

Because it offers the possibility of greater insight and understanding and the hope of creating one's own designs and analyses, this book is naturally somewhat more difficult than conventional texts. But the increased difficulty is not one of mathematical sophistication which many researchers may not have the time nor the inclination to pursue. On the contrary it lies only in the need for disciplined practice to acquire familiarity with the symbolization system, well within the resources of most research psychologists and a small price to pay for the increased insight and research skill which should accrue.

If sufficient research workers adopt it and sufficient methods courses embody it, the result could be an important stride forward in research sophistication in psychology; in this hope it can be highly recommended to all research workers and teachers of psychological statistics.

A. B. ROYSE

Psychometrics of Similarity. By R. A. M. Gregson, London: Academic Press. 1975. Pp. 262. £10.30.

Many potential readers of this work may be misled by the title into supposing that inside they will find much about classification, cluster analysis and multidimensional scaling in all its disguises. Those tempted to make such a hasty judgement would do well to note that the author begins with a quote from Russell's *An Inquiry into the Meaning of Truth* and to take heed from this, since much of this work is concerned with the more philosophical aspects of 'similarity'.

The author's intention is to give a comprehensive guide to similarity as a psychological concept. Certainly this work is comprehensive, but its difficult subject-matter and the relatively high level of mathematics demanded of its reader is likely to restrict readership to those involved in what is, perhaps, one of the less popular areas of psychology. In its field, however, it will obviously prove to be important work.

The text as a whole is beautifully organized, written and produced, and perhaps its major use will be as a reference work with certain sections being consulted rather more often than others. For example, the chapter on 'The Spatial Representation of Similarity' (and here the work of Sheppard, Kruskal, Lingoes, Johnson, and Jardine and Sibson does appear) is likely to have more than a minority interest. The section in chapter 2 on 'Similarity in Classificatory Statistics' (where Sokal and Sneath, Lance and Williams, and Gower make brief appearances) should also appeal to a larger audience than the majority of the book. Other sections in chapter 2 describing early (very early!) work on the concept of similarity do, however, appear to be rather redundant, apart, that is, from indicating the scholarship of the author.

With the present economic climate, prices of books do not seem so surprising as they did only a short time ago, but £10.30 for a volume of this size, despite its obvious importance, does even today seem somewhat excessive.

B. S. EVERITT

Primer of Methods for the Behavioural Sciences. By R. Rosenthal and R. L. Rosnow. New York: Wiley. 1975. Pp. x + 117. £4.20; paper, £1.95.

Introductions to research in the behavioural sciences have a difficult job to do. Ideally they should combine three functions. They should enable the reader (1) to design a reasonable piece of research; (2) to be a critical consumer of the behavioural research literature; and (3) to understand the technical terminology of research methodology.

To attempt to perform all of these functions requires a great deal of space (e.g. Kerlinger's *Foundations of Behavioural Research*), so it is clear that Rosenthal and Rosnow cannot have satisfactorily fulfilled all of them in their short book. What they have done is concentrate on the third, and they have provided a descriptive taxonomy of different types of research with some information on statistical analysis and the (nowadays) inevitable chapter on ethical issues. Advice on ethics is presumably of use to the intending researcher, but the authors have avowedly written for the consumer of behavioural research, so this chapter is of little value. The space devoted to it could have been better used to provide more information on research errors, boobs and faults. Thus in the chapter on problems of inference in research the beginner might be given the impression that the only really serious source of error in behavioural research is observational reactivity – this gets ten out of the twelve pages. While this problem is dealt with very fully and competently it is clear that more space should have been devoted to a systematic exposition of other sources of error, which are merely mentioned in passing in other chapters. Detailed examination of a few detailed examples of famous pieces of erroneous research might have helped, e.g. the Pygmalion effect studies.

But the book has its merits. It indicates, by brief descriptions of intrinsically interesting research studies, the range of research conducted by psychologists. Other virtues include descriptions of cross-lagged data, Mosteller's procedure for standardizing the margins and the Solomon four-group design. Many students will find this work to be of more interest than some of its competitors and will gain something from reading it, but while their ability to describe, categorize and understand research will have been improved, it is not likely that they will be very critical consumers.

P. LEY

Br. J. Psychol. (1975), 66, 4, *pp.* 543–547
Printed in Great Britain

OTHER PUBLICATIONS RECEIVED

(Inclusion in this list does not necessarily preclude later review)

Adams, J. L. *Conceptual Blockbusting: a Guide to Better Ideas.* San Francisco: W. H. Freeman. 1975. Pp. xii + 137. £3.10; paper, £1.70.

Anderson, R. S. (ed.). *Pet Animals and Society.* London: Baillière, Tindall. 1975. Pp. 165. £3.30.

Andreski, S. *Social Sciences as Sorcery.* Harmondsworth: Penguin Books. 1975. Pp. 249. 45p.

Armer, M. *African Social Psychology: a Review and Annotated Bibliography.* African Bibliography Series 2. New York: Holmes & Meir. 1975. Pp. xii + 321. $30.00.

Askenasy, A. *Attitudes towards Mental Patients: a Study across Cultures.* The Hague: Mouton. 1974. Pp. xvi + 324. Dfl. 38. $16.00.

Atkinson, R. C. (ed.). *Psychology in Progress.* London: W. H. Freeman. 1975. Pp. x + 392. £6.60; paper, £3.20.

Bany, M. A. & Johnson, L. V. *Educational Social Psychology.* London: Collier Macmillan. 1975. Pp. xi + 451. £4.95.

Barron, R. *Moral Philosophy for Education.* London: Allen & Unwin. 1975. Pp. 214. £4.95; paper, £2.50.

Barton, A. *Three Worlds of Therapy: an Existential-Phenomenological Approach to the Therapies of Freud, Jung and Rogers.* California: Mayfield Publishing. 1974. Pp. x + 271. $8.95; paper, $4.95.

Bayes, R. *Una Introducción al Método Cientifico en Psicología.* Barcelona: Editorial Fontella. 1974. Pp. 242. Price not given.

Becker, E. *The Revolution in Psychiatry.* London: Collier Macmillan. 1975. Pp. xi + 276. £1.50.

Bergling, K. *The Development of Hypothetico-Deductive Thinking in Children.* Sweden: Almqvist & Wiksell International. 1974. Pp. xix + 127. Sw.kr. 36.

Bernard, J. *Women, Wives and Mothers: Values and Opinions.* Chicago: Aldine. 1975. Pp. vii + 286. $15.00; paper $5.95.

Bonniec, G. P. L. *Le Raisonnement modal: étude génétique.* The Hague: Mouton. 1975. Pp. xviii + 288. Price not given.

Breen, D. *The Birth of a First Child: Towards an Understanding of Femininity.* London: Tavistock. 1975. Pp. xi + 262. £7.00.

Brislin, R. W., Bochner, S. & Lonner, W. J. (eds.). *Cross-cultural Perspectives on Learning.* Chichester: Wiley. 1975. Pp. viii + 336. £8.45.

Brissett, D. & Edgley, C. *Life as Theater: a Dramaturgical Sourcebook.* Chicago: Aldine. 1975. Pp. x + 389. $15.00; paper, $4.95.

Bruch, H. *Learning Psychotherapy: Rationale and Ground Rules.* London: University of Chicago Press. 1975. Pp. xiv + 150. £4.50.

Cancian, F. *What are Norms? A Study of Beliefs and Action in a Maya Community.* London: Cambridge University Press. 1975. Pp. x + 212. £5.50.

Connolly, K. J. & Bruner, J. S. (eds.). *The Growth of Competence.* London: Academic Press. 1974. Pp. xii + 327. £6.80.

Cull, J. G. & Hardy, R. E. *Problems of Disadvantaged and Deprived Youth.* (American Lecture Series.) Springfield, Ill.: Thomas. 1975. Pp. xxi + 248. $13.50.

Cullen, J. W. *Legacies in the Study of Behavior*. Springfield, Ill.: Thomas. 1974. Pp. ix + 275. $17.50.

Davies, A. (ed.). *Problems of Language and Learning*. London: Heinemann. 1975. Pp. vi + 154. £3.00.

Dawson, J. L. M. & Lonner, W. J. *Readings in Cross-Cultural Psychology*. Hong Kong: University Press. Pp. xviii + 397. Price not given.

Eysenck, H. J. *The Future of Psychiatry*. London: Methuen. 1975. Pp. 24. 65p.

Eysenck, H. J., Arnold, W. J. & Meili, R. (eds.). *Encyclopedia of Psychology*. Vol. 1, A–K. London: Fontana. 1975. Pp. xvii + 571. £2.00.

Eysenck, H. J., Arnold, W. J. & Meili, R. (eds.). *Encyclopedia of Psychology*. Vol. 2, L–Z. London: Fontana. 1975. Pp. ix + 1186.

Fantino, E. & Reynolds, G. S. *Introduction to Contemporary Psychology*. London: W. H. Freeman. 1975. Pp. xiv + 610. £6.50.

Fraser, J. T. *Of Time, Passion and Knowledge*. New York: Braziller. 1975. Pp. xiii + 529. $17.95 pre-publication, $20.00 thereafter.

Freedman, D. G. *Human Infancy: an Evolutionary Perspective*. Chichester: Wiley. 1975. Pp. x + 212. £7.00.

Friedman, R. C., Richart, R. M. & Vandewiele, R. L. (eds.). *Sex Differences in Behavior*. Chichester: Wiley. 1974. Pp. xvi + 495. £12.50.

Freud, A. (ed.). *The Psychoanalytic Study of the Child*. Vol. xxix. London: Hogarth Press. 1975. Pp. vii + 544. £6.50.

Freud, S. *The Psychopathology of Everyday Life*. Harmondsworth: Penguin Books. 1975. Pp. 382. 90p.

Gahagan, J. *Interpersonal and Group Behaviour*. London: Methuen. 1975. Pp. 144. 65p.

Garan, D. G. *The Key to the Sciences of Man: the 'Impossible' Relativity of Value Reactions*. New York: Philosophical Library. 1975. Pp. xii + 561. $10.00.

Gazda, G. M. (ed.). *Basic Approaches to Group Psychotherapy and Group Counseling*. Springfield Ill.: Thomas. 1975. Pp. xviii + 535. $16.75.

Giustino, D. de. *Conquest of Mind*. London: Croom Helm. 1975. Pp. viii + 248. £6.00.

Glaser, K. *Learning Difficulties: Causes and Psychological Implications – A Guide for Professionals*. Springfield, Ill.: Thomas. 1974. Pp. xiv + 92. $8.75.

Glick, I. O., Weiss, R. S. & Parkes, C. M. *The First Year of Bereavement*. Chichester: Wiley. 1975. Pp. xvii + 311. £6.75.

Grace, H. K. *The Development of a Child Psychiatric Treatment Program*. Chichester: Wiley. Pp. 244. £6.75.

Green, R. (ed.). *Human Sexuality: a Health Practitioner's Text*. Baltimore: Williams & Wilkins. 1975. Pp. xii + 251. $14.50.

Greene, J. *Thinking and Language*. London: Methuen. 1975. Pp. 144. 65p.

Gregg, L. W. (ed.). *Knowledge and Cognition*. Chichester. Wiley. 1974. Pp. ix + 320. £8.20.

Gross, L. (ed.). *Sexual Behavior: Current Issues*. Chichester: Wiley. 1974. Pp. 291. £7.00, paper, £3.20.

Hamlin, B. *Awareness Experiences for School Use*. Cincinnati: Pflaum/Standard. 1974. Pp. 73. $3.95.

Haslam, M. T. *Psychiatric Illness in Adolescence: its Psychopathology and Progress*. London: Butterworth. 1975. Pp. ix + 121. £5.00.

Heim, A. W. *Psychological Testing*. London: Oxford University Press. 1975. Pp. 16. 30p.

HOSFORD, R. E. & SCOTT MOSS, C. *The Crumbling Walls: Treatment and Counseling of Prisoners.* Illinois: University Press. 1975. Pp. xiv + 257. $8.95.

JOHNSON, W. R. *Sex Education and Counseling of Special Groups: the Mentally and Physically Handicapped, Ill and Elderly.* Springfield, Ill.: Thomas. 1975. Pp. viii + 213. $12.50.

JULIEN, R. M. *A Primer of Drug Action.* London: W. H. Freeman. 1975. Pp. xii + 290. £5.50; paper, £2.50.

KANTOWITZ, B. H. (ed.). *Human Information Processing: Tutorials in Performance and Cognition.* Chichester: Wiley. 1975. Pp. x + 365. £9.75.

KIEV, A. *Magic, Faith and Healing.* London: Collier Macmillan. 1975. Pp. xvii + 475. £1.75.

KING-ELLISON GOOD, P. & BRANTNER, J. P. *A Practical Guide to the MMPI.* London: Oxford University Press. 1975. Pp. 102. £3.45.

KINSBOURNE, M. & LYNN SMITH, W. (eds.). *Hemispheric Disconnection and Cerebral Function.* Springfield, Ill.: Thomas. 1975. Pp. xvi + 300. $21.50.

KINTSCH, W. *The Representation of Meaning in Memory.* Chichester. Wiley. 1975. Pp. vii + 279. £8.00.

KLEIN, M. *Narrative of a Child Analysis: The Conduct of the Psychoanalysis of Children as seen in the Treatment of a Ten-Year-Old Boy.* London: Hogarth Press. 1975. Pp. 496. £5.00.

KLEIN, M. *The Psychoanalysis of Children.* London: Hogarth Press. 1975. Pp. xvi + 326. £5.00.

KLOPFER, W. G. & REED, M. R. (eds.). *Problems in Psychotherapy: an Eclectic Approach.* Chichester: Wiley. 1974. Pp. xi + 178. £6.45.

KOZLOFF, M. A. *Educating Children with Learning and Behaviour Problems.* Chichester: Wiley. 1974. Pp. xix + 459. £9.20.

KREEGER, L. (ed.). *The Lage Group.* London: Constable. 1975. Pp. 344. £5.00; paper, £2.50.

LAUGHLIN, C. D. & D'AQUILI, E. G. *Biogenetic Structuralism.* London: Columbia University Press. 1975. Pp. 211. £5.00.

LEGGE, D. *An Introduction to Psychological Science.* London: Methuen. 1975. Pp. 144. 65p.

LEWIS, M. & ROSENBLUM, L. A. (eds.). *The Origins of Fear.* Vol. 2. Chichester. Wiley. 1975. Pp. xiii + 284. £9.70.

LISKA, A. *The Consistency Controversy: Readings on the Impact of Attitude on Behaviour.* Chichester: Wiley. 1975. Pp. viii + 277. £6.00; paper, £3.20.

LONDON, P. *Beginning Psychology.* Homewood, Ill.: Dorsey Press. 1975. Pp. xvi + 655. Price not given.

LOVELL, M. *How Children Grow. I: From Conception to Two.* London: Routledge & Kegan Paul. 1975. Pp. x + 130. £2.95.

MARGOLIS, B. L. & KROES, W. H. (eds.). *The Human Side of Accident Prevention.* Springfield, Ill.: Thomas. 1975. Pp. ix + 144. $11.50.

MASSONS, J. M. D. *Metodos Estadisticos Parala Investigacion et Ciencias Humanas.* Barcelona: Editorial Herder. 1975. Pp. 450. 850 ptas.

MENDELSON, M. *Psychoanalytic Concepts of Depression.* (Second edition.) Chichester: Wiley. 1975. Pp. xiv + 352. £8.00.

MILMAN, D. S. & GOLDMAN, G. D. *Group Processs Today: Evaluation and Perspective.* Springfield: Ill.: Thomas. 1974. Pp. xxii + 313. $14.75.

MEYERSON, S. (ed.). *Adolescence and Breakdown: a Study of Adolescence by Members of the Tavistock Clinic and other British Experts.* London: Allen & Unwin. 1975. Pp. 140. £3.95; paper, £1.95.

Meyerson, S. (ed.). *Adolescence and the Crises of Adjustment: A Study of Adolescence by Members of the Tavistock Clinic and other British Experts*. London: Allen & Unwin. 1975. Pp. 189. £4.25; paper, £2.35.

Mitchell, J. *Psychoanalysis and Feminism*. Harmondsworth. Penguin Books. 1975. Pp. xxiii + 456. £1.20.

Nuttin, J. M. Jr. *The Illusion of Attitude Change Towards a Response Contagion Theory of Persuasion*. London; Academic Press. 1975. Pp. xii + 236. £6.40.

Parkes, C. M. *Bereavement: Studies of Grief in Adult Life*. Harmondsworth; Penguin Books. 1975. Pp. 267. 80p.

Pask, G. *The Cybernetics of Human Learning and Performance*. London: Hutchinson Educational. 1975. Pp. 346. £7.75.

Pasternak, S. A. (ed.). *Violence and Victims*. Chichester: Wiley. 1975. Pp. xii + 215. £8.00.

Phill, E. J. *Invention and Discovery of Reality*. Chichester: Wiley. 1975. Pp. 203. £6.50.

Peine, H. A. & Howarth, R. *Children and Parents: Everyday Problems of Behaviour*. Harmondsworth: Penguin Books. 1975. Pp. 133. 50p.

Pick, A. D. (ed.). *Minnesota Symposia on Child Psychology*. Vol. 8. London: Oxford University Press. 1975. Pp. viii + 197. £5.50.

Pringle, M. K. *The Needs of Children* London: Hutchinson Educational. 1975. Pp. 190. £1.45.

Reich, W. *The Murder of Christ*. London: Souvenir Press. 1975. Pp. 227. £3.00; paper, £1.75.

Reik, T. *Of Love and Lust*. London: Souvenir Press. 1975. Pp. xiv + 623. £5.00; paper, £2.60.

Rommetveit, R. *On Message Structure: a Framework of the Study of Language and Communication*. Chichester: Wiley. 1974. Pp. 143. £4.95.

Rose, G. & Marshall, T. F. *Counselling and School Social Work*. Chichester: Wiley. 1975. Pp. vii + 347. £6.50.

Rosenthal, R. & Rosnow, R. L. *The Volunteer Subject*. Chichester: Wiley. 1975. Pp. xiv + 266. £5.40.

Sandperl, I. *A Little Kinder*. Palo Alto: Science & Behavior. 1975. Pp. xi + 163. $5.95.

Sandven, J. *Projectometry*. Oslo: Universitetsforlaget. 1975. Pp. 372. Nkr. 90. $18.00.

Scherer, K. R., Abeles, R. P. & Fischer, C. S. *Human Aggression and Conflict*. Englewood Cliffs, N.J.: Prentice-Hall. 1975. Pp. x + 337. Price not given.

Scroth, M. L. & Sue, D. W. *Introductory Psychology*. Homewood, Ill.: Dorsey Press. 1975. Pp. xii + 516. Price not given.

Seligman, M. E. P. *Helplessness on Depression, Development and Death*. London: W. H. Freeman. 1975. Pp. xv + 250. £4.70; paper, £2.40.

Shakespeare, R. *The Psychology of Handicap*. London: Methuen. 1975. Pp. 143. 65p.

Shapiro, K. J. & Alexander, I. E. *The Experience of Introversion: an Integration of Phenomenological, Empirical and Jungian Approaches*. N. Carolina: Duke University Press. 1975. Pp. xii + 180. $8.75.

Sheldon, K. *The Hanged Man: Psychotherapy and the Forces of Darkness*. Palo Alto: Science & Behavior. 1975. Pp. 255. $7.95.

Sheridan, T. B. & Ferrell, W. R. *Man–Machine Systems, Information, Control and Decision Models of Human Performance*. London: University of Chicago Press. 1975. Pp. ix + 452. £12.50.

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SMITH, A. *Towards Behavioural Science: Essays on Foundations and Methods*. London: Panparameter. 1975. Pp. 207. £4.80.

STEPHENSON, E. D. (ed.). *Gestalt Therapy Primer: Introductory Readings in Gestalt Therapy*. Springfield, Ill.: Thomas. 1975. Pp. xvi + 216. $16.50.

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TEYLOR, T. J. *A Primer of Psychobiology: Brain and Behaviour*. New York: W. H. Freeman. 1975. Pp. xii + 143. $3.50.

TULIPAN, A. B., ATTNEAVE, C. L. & KINGSTONE, E. (eds.). *Beyond Clinical Walls*. Alabama: University of Alabama Press. 1975. Pp. xii + 241. $8.50.

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WHELDALL, K. *Social Behaviour*. London: Methuen. 1975. Pp. 144. 65p.

WHITING, H. T. A. & MASTERSON, D. W. (eds.). *The Human Movement Series: Readings in the Aesthetics of Sport*. London: Lepus. 1974. Pp. xiv + 160. Price not given.

WILSON, B. R. *Education, Equality and Society*. London. Allen & Unwin. 1975. Pp. 207. £4.25.

YOLLES, S. F., CARONE, P. A. & KRINSKY, L. W. *Absenteeism in Industry* Springfield, Ill.: Thomas. 1974. Pp. xvii + 141. $11.50.

ZELKIND, I. & SPRUG, J. *Time Research*. 1172 *Studies*. Metuchen, N.J.: Scarecrow Press, London: Bailey Brothers & Swinfen.

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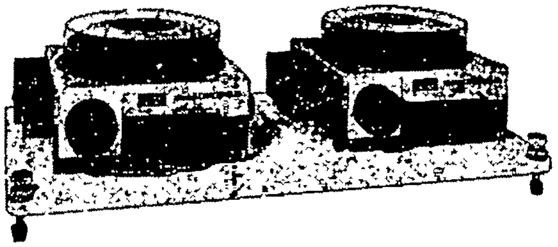

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B. Choppin: Guessing the Answer on Objective Tests.

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MAIN, T. F. A few psychodynamic features of enlarged groups.
NAPOLITANI, F. The group as a therapeutic agent.
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NAPOLITANI, D. Maturation stages of the psychoanalytic group: mutual phantasmatic indications between analyst and group.
NAPOLITANI, F. – ANCONA, L. Psychoanalytically-directed group processes in terms of Kleinian identifications.
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CONTENTS OF VOLUME 66

Part 1 February 1975

PAGE

Diurnal variation in logical reasoning. By Simon Folkard 1

Extraversion and variety seeking in a monotonous task. By A. B. Hill . 9

Extraversion, neuroticism and verbal reasoning ability as determinants of paired-associates learning. By J. F. Allsop and H. J. Eysenck . . 15

Colours experienced in dreams. By Charles A. Padgham 25

An investigation of the use of the Photo-fit technique for recalling faces. By Hadyn Ellis, John Shepherd and Graham Davies. 29

Handedness and controlled movement. By Kenneth Flowers . . . 39

Retest reliabilities and interrelationship of the Annett Hand Preference Questionnaire and the Edinburgh Handedness Inventory. By Elizabeth R. L. McMeekan and W. A. Lishman 53

The effects of noise and incentives upon attention in short-term memory. By D. R. Davies and D. M. Jones 61

'Perceptual' strategies in the solving of three-term series problems. By Graham Quinton and Brian J. Fellows 69

An algorithm for the segmentation of an artificial language analogue. By J. G. Wolff 79

Effect of length, density and angle between arms on gestalt grouping. By Lubomir S. Prytulak and Delbert A. Brodie 91

Temporal numerosity discrimination: intermodal comparisons revisited. By Eugene C. Lechelt 100

Book reviews 109

Other publications received 127

Part 2 May 1975

A check on hypothetical personality structures and their theoretical interpretation at 14–16 years in T-data. By Raymond B. Cattell and Thomas W. Klein 131

The role of stimulus and pair imagery in paired-associate learning. By P. E. Morris and R. L. Reid 153

The effects of different modes of supervision on vigilance behaviour. By Vernon R. Putz 157

Note on the Craik–O'Brian effect. By S. Kreekling . . . 161

PAGE

A note on interactions of ears, hands and cerebral hemispheres in a musical performance task requiring bimanual coordination. By ANNE GATES and JOHN L. BRADSHAW 165

Assimilative projection and construct balance in the repertory grid. By J. BENJAFIELD and J. ADAMS-WEBBER 169

Uric acid and divergent thinking: a possible relationship. By K. F. KENNETT and A. J. CROPLEY 175

A study of assisted problem-solving. By DAVID WOOD and DAVID MIDDLETON 181

Effects of tactual and phonological similarity on the recall of Braille letters by blind children. By SUSANNA MILLAR 193

The recall of digits by normal, deaf and autistic children. By B. HERMELIN and N. O'CONNOR 203

Stimulus generalization following extra-dimensional training in educationally subnormal (severely) children. By J. HOGG and P. L. C. EVANS . . 211

A simple two-group test for matched scores with unequal cell frequencies. By R. MEDDIS. 225

Limitations on temporal control: generalization and the effects of context. By J. E. R. STADDON 229

Book reviews 247

Other publications received 271

PART 3 AUGUST 1975

Subjective value of information. By ALAN BRANTHWAITE 275

Information complexity and cross-modal functions. By DAVID FREIDES . 283

Stimulus complexity, EEG abundance gradients, and detection efficiency in a visual recognition task. By ANTHONY GALE, GRAHAM SPRATT, BRUCE CHRISTIE and ADRIAN SMALLBONE 289

Anxiety and ritualized speech. By MANSUR LALLJEE and MARK COOK . . 299

A note on inappropriate constancy-scaling and the Müller-Lyer illusion. By ALAN GAULD 307

The effects of negative relatedness upon word-picture and word-word comparisons and subsequent recall. By ANGUS R. H. GELLATLY and VERNON H. GREGG 311

Context-dependent memory in two natural environments: on land and underwater. By D. R. GODDEN and A. D. BADDELEY 325

Imagery and deep structure in the recall of English nominalizations. By JOHN T. E. RICHARDSON 333

PAGE

Visual imagery and the discrimination of differences between altered pictures simultaneously and successively presented. By RUBEN C. GUR and ERNEST R. HILGARD 341

The subliminal perception of movement and the 'suppression' in binocular rivalry. By PETER WALKER 347

Experimenter bias and subliminal perception. By PAUL J. BARBER and J. PHILLIPPE RUSHTON 357

Context, meaning and the validity of results in psychological research. By ARIE W. KRUGLANSKI 373

Research note: a new technique for studying phenomenal causality. By D. G. BOYLE 383

Book reviews 387

Other publications received 405

PART 4 NOVEMBER 1975

Recognizing faces. By HADYN D. ELLIS 409

The semantics of locative information in pictures and mental images. By MARCEL ADAM JUST and PATRICIA A. CARPENTER 427

Choice of non-reinforcement following forced alternation. By DENNIS H. HOLDING, JOHN C. BIRKIMER, KENNETH R. DANIELS and ROBERT P. BROWN 443

Spatial memory by blind and sighted children. By SUSANNA MILLAR . . 449

Spatial perception in the blind. By BILL JONES 461

Developmental changes in consensus in construing within a specified domain. By ARTHUR APPLEBEE 473

Preferences for common words, uncommon words and non-words by children and young adults. By A. M. COLMAN, M. WALLEY and W. SLUCKIN . 481

Degrees of novelty and their effects on children's attention and preference. By CORINNE HUTT 487

The effects of chronological age on the relationship of intelligence and academic achievement with extraversion and neuroticism. By G. M. SEDDON . 493

An analysis of the items of the Eysenck personality inventory. By K. D. MACRAE and R. P. POWER 501

Personality and differential susceptibility to hypnosis: further replication and sex differences. By H. B. GIBSON and M. E. CORCORAN 513

Book reviews 521

Other publications received 543

Editorial acknowledgement 549

INDEX OF AUTHORS

	PAGE
Adams-Webber, J.	169
Allsopp, J. F.	15
Applebee, A. N.	473
Baddeley, A. D.	325
Barber, P. J.	357
Benjafield, J.	169
Birkimer, J. C.	443
Boyle, D. G.	383
Bradshaw, J. L.	165
Branthwaite, A.	275
Brodie, D. A.	91
Brown, R. P.	443
Carpenter, P. A.	427
Cattell, R. B.	131
Christie, B.	289
Colman, A. M.	481
Cook, M.	299
Corcoran, M. E.	513
Cropley, A. J.	175
Daniels, K. R.	443
Davies, D. R.	61
Davies, G.	29
Ellis, H.	29, 409
Eysenck, H. J.	15
Evans, P. L. C.	211
Fellows, B. J.	69
Flowers, K.	39
Folkard, S.	1
Freides, D.	283
Gale, A.	289
Gates, A.	165
Gauld, A.	307
Gellatly, A. R. H.	311
Gibson, H. B.	513
Godden, D. R.	325
Gregg, V. H.	311
Gur, R. C.	341
Hermelin, B.	203
Hilgard, E. R.	341
Hill, A. E.	9
Hogg, J.	211
Holding, D. H.	443
Hutt, C.	487
Jones, B.	461
Jones, D. M.	61
Just, M. A.	427
Kennett, K. F.	175
Klein, T. W.	131
Kreekling, S.	161
Kruglanski, A. W.	373
Lalljee, M.	299
Lechelt, E. C.	100
Lishman, W. A.	53
MacRae, K. D.	501
McMeekan, E. R. L.	53
Meddis, R.	225
Middleton, D.	181
Millar, S.	193, 449
Morris, P. E.	153
O'Connor, N.	203
Padgham, C. A.	25
Power, R. P.	501
Prytulak, L. S.	91
Putz, V. R.	157
Quinton, G.	69
Reid, R. L.	153
Richardson, T. E.	333
Rushton, J. P.	357
Seddon, G. M.	493
Shepherd, J.	29
Sluckin, W.	481
Smallbone, A.	289
Spratt, G.	289
Staddon, J. E. R.	229
Walker, P.	347
Walley, M.	481
Wolff, J. G	79
Wood, D.	181

INDEX OF PUBLICATIONS REVIEWED

Adolfson, J. A. & Berghage, T. E. *Perception and Performance Under Water* 123

Atkinson, J. W. & Raynor, J. O. *Motivation and Achievement* 120

Becker, J. *Depression: Theory and Research* 264

Beloff, J. *New Directions in Parapsychology* 111

Biller, H. B. *Paternal Deprivation* 399

Blackman, D. *Operant Conditioning: an Experimental Analysis of Behaviour* 261

Bordin, E. S. *Research Strategies in Psychotherapy* 532

Brenner, H. *Mental Illness and the Economy* 116

Broughton, W. B. *The Biology of Brains* 256

Brown, R. *A First Language* 249

Bruner, J. S. *Beyond the Information Given* 247

Carpenter, F. *The Skinner Primer: Behind Freedom and Dignity* 109

Cranach, M. von & Vine, I. (eds.). *Social Communication and Movement* 392

Cratty, B. J. *Psychomotor Behaviour in Education and Sport* 268

Dement, W. C. *Some Must Watch While Some Must Sleep* 266

Deutsch, M. *The Resolution of Conflict: Constructive and Destructive Processes* 119

Eisdorfer, C. & Lawton, M. P. *The Psychology of Adult Development and Aging* 252

Epstein, A. N. *et al.* (eds.). *The Neuropsychology of Thirst: New Findings and Advances in Concepts* 256

Essman, W. B. & Nakajima, S. (eds.). *Current Biochemical Approaches to Learning and Memory* 125

Eysenck, H. J. *The Measurement of Intelligence* 394

Fink, M. *et al.* (eds.). *Psychobiology of Convulsive Therapy* 265

Fisher, S. & Freedman, A. M. *Opiate Addiction: Origins and Treatment* 263

Forrest, D. W. *Francis Galton: the Life and Work of a Victorian Genius* 387

Gregory, R. L. *Concepts and Mechanisms of Perception* 259

Gregson, R. A. M. *Psychometrics of Similarity* 540

Hardy, R. E. & Cull, J. G. (eds.). *Problems of Adolescents: Social and Psychological Approaches* 531

Hardy, R. E. & Cull, J. G. (eds.). *Psychological and Vocational Rehabilitation of the Youthful Delinquent* 531

Hartman, E. L. *The Functions of Sleep* 396

Himmelfarb, S. & Eagly, A. H. (eds.). *Readings in Attitude Change* 120

Hudson, L. *The Cult of Fact* 253

Hughes, A. *Psychology and the Political Experience* 526

Junter, J. F. M. *Essays after Wittgenstein* 114

Jeeves, M. *Experimental Psychology: an Introduction for Biologists* 403

Joynson, R. B. *Psychology and Common Sense* 109

Kamin, L. J. *The Science and Politics of I.Q.* 525

Kaufman, L. *Sight and Mind: an Introduction to Visual Perception* 391

Kline, P. (ed.). *New Approaches in Psychological Measurement* 538

Lee, W. *Experimental Design and Analysis* 539

Lester, D. *A Physiological Basis for Personality Traits: a New Theory of Personality* 397

Leuba, J. H. *The Psychology of Religious Mysticism* 112

Levy, L. & Rowitz, L. *The Ecology of Mental Disorder* 116

Loehlin, J. C. *et al. Race Differences in Intelligence* 521

Malikin, D. (ed.). *Social Disability: Alcoholism, Drug Addiction, Crime and Social Disadvantage* 393

Mark, R. *Memory and Nerve Cell Connections* 125

Markin, R. J. *Consumer Behaviour: a Cognitive Orientation* 528

Mauss, M. *A General Theory of Magic* 112

Meyers, L. S. & Grossen, N. E. *Behavioural Research: Theory, Procedures and Design* 260

Michael, R. P. & Crook, J. H. (eds.). *Comparative Ecology and Behaviour of Primates* 269

Michie, D. *On Machine Intelligence* 115

Miles, T. R. *The Dyslexic Child* 403

Milgram, S. *Obedience to Authority* 524

Minuchin, S. *Families and Family Therapy* 399

Mischel, T. (ed.). *Understanding Other Persons* 110

Modgil, S. *Piagetian Research: a Handbook of Recent Studies* 121

Morris, J. *Conundrum* 262

Neulinger, J. *The Psychology of Leisure* 267

Naumberg, M. *An Introduction to Art Therapy: Studies of the 'Free' Art Expression of Behaviour Problem Children and Adolescents as a Means of Diagnosis and Therapy* 118

Newton, G. & Riesen, A. H. (eds.). *Advances in Psychobiology*, vol 2 533

Osherson, D. N. *Logical Abilities in Children* 402

Powers, W. T. *Behaviour: the Control of Perception* 389

Pringle, M. K. & Varma, V. P. *Advances in Educational Psychology* 398

Reitan, R. M. & Davison, L. A. (eds.). *Clinical Neuropsychology: Current Status and Affiliations* 258

Róheim, G. *Animism, Magic and the Divine King* 112

Rosenthal, R. & Rosnow, R. L. *Primer of Methods for the Behavioural Sciences* 540

Ross, H. E. *Behaviour and Perception in Strange Environments* 124

ROUTH, D. K. *The Experimental Psychology of Mental Retardation* 251

SATTLER, J. M. *Assessment of Children's Intelligence* 122

SAUBER, S. R. *Preventive Educational Intervention for Mental Health* 401

SAVAGE, R. D. *et al. Intellectual Functioning in the Aged* 395

SCHACHTER, S. & RODIN, J. *Obese Humans and Rats* 536

SIEGEL, L. & LANE, I. M. *Psychology in Industrial Organizations* 527

STAATS, A. W. *Social Behaviorism* 522

STEIN, D. G. & ROSEN, J. J. (eds.). *Basic Structure and Function in the Nervous System* 534

STEIN, D. G. & ROSEN, J. J. (eds.). *Motivation and Emotion* 534

STEIN, D. G. & ROSEN, J. J. (eds.). *Learning and Memory* 534

STONE, J. L. *et al.* (eds.). *The Competent Infant* 248

STONE, W. F. *The Psychology of Politics* 527

STRUPP, H. H. *et al.* (eds.). *Psychotherapy and Behaviour Change* 390

TEDESCHI, J. T. (ed.). *Perspectives on Social Power* 527

THOULESS, R. H. *From Anecdote to Experiment in Psychical Research* 112

TOATES, F. *Control Theory in Biology and Experimental Psychology* 536

VARMA, V. (ed.). *Psychotherapy Today* 117

VYGOTSKY, L. S. *The Psychology of Art* 404

WASSERMAN, G. D. *Brains and Reasoning* 388

WHITE, N. F. (ed.). *Ethology and Psychiatry* 530

WILKERSON, T. E. *Minds, Brains and People* 255

WILLIAMS, R. L. *et al. Electroencephalography (EEG) of Human Sleep: Clinical Applications* 266

WING, J. K. *et al. The Measurement and Classification of Psychiatric Symptoms* 396

WINICK, C. *Sociological Aspects of Drug Dependence* 532

ZAX, M. & SPECTER, G. A. *An Introduction to Community Psychology* 529

5 NOV

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The British Journal of Psychology *Volume 66, Part 4, November 1975*

CONTENTS

ELLIS, HADYN D.
Recognizing faces *page* 409

JUST, MARCEL ADAM AND CARPENTER, PATRICIA A.
The semantics of locative information in pictures and mental images 427

HOLDING, DENNIS H., BIRKIMER, JOHN C., DANIELS, KENNETH R. AND BROWN, ROBERT P.
Choice of non-reinforcement following forced alternation 443

MILLAR, SUSANNA
Spatial memory by blind and sighted children 449

JONES, BILL
Spatial perception in the blind 461

APPLEBEE, ARTHUR
Developmental changes in consensus in construing within a specified domain 473

COLMAN, A. M., WALLEY, M. AND SLUCKIN, W.
Preferences for common words, uncommon words and non-words by children and young adults 481

HUTT, CORINNE
Degrees of novelty and their effects on children's attention and preference 487

SEDDON, G. M.
The effects of chronological age on the relationship of intelligence and academic achievement with extraversion and neuroticism 493

MacRAE, K. D. AND POWER, R. P.
An analysis of the items of the Eysenck personality inventory 501

GIBSON, H. B. AND CORCORAN, M. E.
Personality and differential susceptibility to hypnosis: further replication and sex differences 513

BOOK REVIEWS 521

OTHER PUBLICATIONS RECEIVED 543

EDITORIAL ACKNOWLEDGEMENT 549

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